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METHODOLOGICAL ISSUES IN MONETARY ECONOMICS

BERNARD GAUCI

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METHODOLOGICAL ISSUES IN MONETARY ECONOMICS

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DISSERTATION

Submitted to the University of New Hampshire
in Partial Fulfillment of
the Requirements for the Degree of

Doctor of Philosophy
in
Economics

May, 1981

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Dedicated to the memory of
Dun Manwel Grima, and to the
memory of Judge Joseph Flores.

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ABSTRACT

METHODOLOGICAL ISSUES IN MONETARY ECONOMICS

by

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University of New Hampshire, May, 1981

The application of the work of I. Lakatos to economics suggests that the neo-Walrasian monetary model is generated from a neoclassical microeconomic research program, as is the rational expectations hypothesis. At the macroeconomic level, there are competing Keynesian and conservative programs. The latter contains monetarism as a constituent set of theories.

Methodological and policy-related debates reflect competition between these three programs. Despite the similarity between the policy recommendations of monetarism and the rational expectations hypothesis, their theoretical apparatus--dictated by the positive heuristic of their respective programs--are substantially different. Monetarism shares its macroeconomic equipment with Keynesianism, but the two differ in their policy recommendations.

A major characteristic of the neoclassical microeconomic program is the situational determinism found in its member theories. This in turn follows from the substantive (rather than procedural) rationality displayed by all economic agents in these theories. The same can be

said of the rational expectations hypothesis. On the other hand, macroeconomic theories, whether Keynesian or monetarist, display a situational determinism only at the level of the policy-maker. The separation of economics from the other social sciences leads to a situational logic which enables the policy-maker in these models to reach decisions through the use of the substantive form of rationality.

INTRODUCTION

The literature has recently shown a proliferation of interest in the methodology of economics. Economics is commonly regarded as a leader among the social sciences in the rigor of its mathematical formulations and also because of the pioneering work in non-experimental corroboration by econometricians. As a result, economics has served as a fertile ground for the application of a variety of approaches in the philosophy of science.

A major recent development in the methodological literature has involved Imre Lakatos' concept of competing scientific research programs. This concept requires the condensation of an entire school of thought to a set of propositions incorporating the fundamental principles of the program together with a set of instructions for the construction of theories. The method used in the dissertation is to present these essential ingredients for one program and later return to ascertain whether a particular theory is compatible with these same ingredients. If not, a new program is suggested in which the theory is then placed as a constituent member.

The concept of the research program is discussed and evaluated in the first chapter, as is the formulation of a neoclassical research program as suggested by S. J. Latsis. The latter's work is discussed further in Chapter 2, in the context of an application to neoclassical monetary theory. The dissertation goes on, in Chapter 3, to justify and then construct a separate Keynesian research program, which itself

competes with the neoclassical microeconomic program. Whether monetarism constitutes a separate program, or whether it belongs in some larger program, is one of the subjects discussed in Chapter 4, which deals with this question of identifying the important characteristics of monetarism, and studying its links with other schools of thought. The conclusion is reached that within the macroeconomic arena, the Keynesian program competes with a conservative program. A separate section of Chapter 4 deals with the rational expectations hypothesis (REH), which is contrasted with monetarism, and, like monetarism, placed in the appropriate research program. The REH is described as appertaining to the neoclassical microeconomic program. This dissertation therefore identifies two levels of program competition: between microeconomic and macroeconomic programs and, among macroeconomic programs, between the Keynesian and conservative programs.

This is primarily an application of the Lakatosian method to Keynesian and monetarist theories as well as the REH, with emphasis throughout on monetary analysis. It finds particular benefit in contrasting the psychological assumptions underlying neoclassical, Keynesian and monetarist theory and the REH. The relevance of this approach to an understanding of monetary analysis is evaluated in the concluding chapter.

CHAPTER 1

ECONOMIC METHOD

The process of building theoretical models in economics or elsewhere is variously described as an "abstraction" from, or "idealization" of reality. Theory can be defined as the symbolic (or, more loosely, the linguistic, mental or verbal) reproduction of what is perceived. The ideal, but paradoxically the least useful, is the theory which constitutes a mirror image of what the perceiver attempts to portray. Limitations of the intellectual capabilities of the theorist stop the process well short of such a state of perfection. Indeed if intellectual powers were perfect, symbolic reproduction in the form of theoretical models would be unnecessary. Instead, the theorist has to make do with a less than perfect intellect and must apply his inadequate equipment to a complex subject matter. So he will limit himself to a portion of his surroundings.

At the risk of ignoring significant relationships, the economist, for example, will often exclude institutional or other social detail from his purview. This procedure, which one may label decomposition, after Simon,¹ is manifested in the division of the social sciences into economics, sociology, anthropology, history and so on, with little substantial interaction among these fields. It is also evident within economics where, for example, the microtheorist ignores macroeconomic variables and vice versa, where the macrotheorist ignores microeconomic detail, or where the monetary theorist assumes a closed economy. Such

a procedure may, in some instances, indicate a narrow-mindedness on the part of the theorist, but is otherwise excused by the inadequacy of the intellectual powers of the theorist. In other words, decomposition may be the only way to make a given problem tractable. But the cost of achieving this easy-way-out is the risk of inaccuracy, or erroneous decomposition. In some instances, the theorist may find himself in a situation where the only reproduction he can make is distorted.

Among the admitted variables, the theorist will emphasize some relationships and play down or ignore others. Once again, the motivation may be merely a need to make a symbolic reproduction possible. For example, in Friedman's interpretation of macroeconomic theory, the Keynesian school ignores the determination of the price level and assumes a fixed price level.² (The quantity theorist adopts a similar procedure for the level of output, although here the level of output can be said to be determined by the separate, general-equilibrium model.) This process may be called specification, with the inherent danger, of course, being that of misspecification.

Sometimes, then, a distorted theory may be the only theory attainable. That a distorted representation of reality is better than no representation at all, or that one distorted presentation may be more useful, and therefore better than another appears to be similar to the position of such instrumentalists as Milton Friedman. What determines the validity of a theory is not the realism of its assumptions, but rather the accuracy of its predictions. Friedman explains:

A hypothesis is important if it "explains" much by little, that is, if it abstracts the common and crucial elements from the mass of complex and detailed circumstances surrounding the phenomena to be explained and permits valid predictions on the

basis of them. To be important, therefore, a hypothesis must be descriptively false in its assumptions; it takes account of, and accounts for, none of the many other attendant circumstances, since its very success shows them to be irrelevant for the phenomena to be explained.³

This, in effect, is Friedman's justification for the techniques of decomposition and specification.

Limitations on the intellectual power of the individual theorist also require most, if not all, theoretical activity to be carried out not by theorists in isolation, but by theorists as part of communities of intellectuals. If, for example, at the age of 35, somebody decides to start studying economics, it is unlikely that he would attempt to develop his own theory ab initio, but rather he would try to build upon the work of his predecessors. In the light of our earlier discussion, it is by the very definition of economics almost impossible to really start from scratch, since the very boundaries of economics were developed by several generations of decompositional activity and are unlikely to coincide with one's own decompositional tactics.

The idea of collegiality among scientists gained added recognition after T. Kuhn's The Structure of Scientific Revolutions.⁴ So-called normal science is carried out within a community which professes allegiance to a so-called paradigm. The meaning of this last term is anything but clear, but can be defined for our purposes as agreed and unquestioned rules about decomposition and specification. These rules severely limit the range of questions addressed by the members of the paradigmatic community. The inability of the scientist to solve any of the puzzles permitted by the paradigm will reflect poorly not on the profession, but on the scientist. In other words, the science with its intrinsic methods of specification and decomposition is absolved from

the problems and errors which these very methods may cause.

By Popper's criteria,⁵ science progresses through a chain of conjectures and refutations. The advance of science is abetted by the construction of theories which lend themselves to refutation. Kuhn believes, however, that the Popperian overthrow of theories occurs only at intermittent stages. It never happens during periods of normal science. The inability of scientists to solve a puzzle here and a puzzle there does not shake the faith, but an accumulation of such anomalies may lead to a switch to a new paradigm. Such an event is called a revolution, where a new and incommensurable paradigm comes to rule the roost. The abruptness of the switch, hardly evident in the slow pace of change in the natural sciences, was toned down in the later Kuhn to allow for an interregnum of pretender-paradigms.⁶

The paradigm, then, is the hallmark of true science, distinguished by a unity of purpose and method in the profession. This is a statement of what science is, but there is a hint of how scientists should act, in that in a paradigmatic science research is streamlined in a fruitful direction. There is no duplication of research efforts, since paradigmatic education enables scientists to communicate with ease. In contrast, during the preparadigmatic as well as the revolutionary period, there is a plethora of research lines, all independent and lacking channels of intercommunication. Paradigmatic science can therefore be called efficient. On the other hand, it could be said to constitute a dictator's paradise. If the dictator executes all the dissidents, those who survive will be scientists in the true Kuhnian sense of the word. In other words, while descriptively the Kuhnian model might perform adequately, ominous results might follow if a college dean were to regard

it as a source of prescriptive (or normative) inspiration.⁷

Yet even insofar as mere description is concerned, problems arise in application to economics. First of all, there is the question of the geographical scope of the paradigm. For example, in some of the natural sciences there might be one universal paradigm, but hardly in economics, where the Soviet paradigm, for example, is entirely different from the American paradigm. It appears, then, that the paradigm in economics may be less than universal. Clearly, the majority of American and British economists, with significant number of continental Europeans, think alike, as it were, in a number of important ways. While the paradigm cannot claim the membership of all the economists on earth, the presence of Marxists, radicals and so on in the United States does not imply that the concept of the paradigm cannot be applied at all. These minorities are well aware of the overwhelming predominance and power of the majority, are conversant with the opinions of the majority, and a significant portion of their academic activity consists precisely of defending their position against the majority opinion.

Then what constitutes the present-day paradigm in western economics? We might start by trying to identify all that is common to the majority. Superficially, we can identify a body of knowledge learned by every successful undergraduate, consisting of neoclassical price theory, particularly the perfectly competitive model, and macroeconomic theory, including primarily basic Keynesian theory. Around the basic, perfectly competitive model are derived rules for social optimization, and models of imperfect competition. Around the macroeconomic core are built frameworks for the formulation and implementation of fiscal and monetary policy. The various parts may be contradictory at times, but a certain

unity emerges in the flavor of policy recommendations. Dominant political thought among economists holds to the underlying desirability of allowing market forces to dominate the economy, with all the benefits of such a policy as suggested by microeconomic welfare theory. That is as much as can be said to be true of the beliefs of the mainstream. A monetarist is likely to hold the view with a very strong conviction; a Keynesian would probably be more willing to propose state intervention. Yet to both, operation of free or relatively free markets in significant sections of the economy is acceptable and to both positions (as well as to intermediate positions), paradigmatic economics provides the necessary theoretical background. Both parties can undergo a common intellectual training, and have a common language in which to debate their paradigmatically-constrained differences. This language includes definitions of a wide range of theoretical terms and econometric techniques. In addition, the professionalism of the science precludes, to a considerable extent, the development and diffusion of non-paradigmatic economics. On the other hand, the further elaboration and testing of intra-paradigmatic issues is encouraged and professionally stimulated.

This method of identifying the paradigm is based on a particular view of economics. Rather than regard theoretical method and empirical investigation as logically, if not chronologically, preceding policy recommendations, I view method in economic theory, as well as econometric method, as following from the school of thought which is itself defined by the social, political or ideological flavor of its policy recommendations. Kuhn's ideas cast doubt on the claim of some econometricians to be the ultimate arbiters in the development of economic theories. As long as theorists and econometricians are inhibited by the paradigm,

the process of corroboration is not totally unconstrained.⁸

Lakatos' Scientific Research Program (SRP)

But what has been gained by viewing all of economics as being encompassed by one paradigm? Clearly, by our approach, a variety of policy recommendations may well require a corresponding variety of theories. Instead of lumping the Keynesians and the monetarists together, it might be more useful to recognize that the two schools compete with one another, even though there is some overlap between them. The only way two schools can coexist in the Kuhnian framework is if the science lies in a nonnormal state.

These kinds of issues are addressed by Lakatos.⁹ Instead of the monolithic Kuhnian paradigm, Lakatos envisages competing research programs, each of which may contain any number of theories. For a theory to belong to a particular program, it must be built upon a set of propositions which constitute the hard core of the program. Unlike the rather vague concept of the "unwritten, but always-observed rules" of the Kuhnian paradigm, the negative heuristic of the SRP instructs the theorist to uphold the specific propositions of the hard core. These propositions can be identified as the decompositional and specificational instructions mentioned earlier in this chapter.

Over time, all the theories appertaining to a program will retain all the propositions of the hard core, but any additional propositions will vary from theory to theory. These extra propositions form the protective belt of the program. Another essential ingredient of a program is the positive heuristic which gives the theoretician his methodological instructions. Allegiance to the positive heuristic of the

program, together with the negative heuristic itself, seem to make up the Lakatosian equivalent of the Kuhnian paradigm.

The program will progress if the modifications incorporated in the protective belt lead to new testable hypotheses and then if this additional content is corroborated. Whenever a theory is not corroborated, scientists are likely to resort to changes in the protective belt and modifications which fail the empirical test will indicate to the methodologist that the program is degenerating.

One benefit of the Lakatosian over the Kuhnian method is that the SRP approach calls for the condensation of a school of thought to a set or sets of propositions. This has the advantage of requiring the theorist to describe theories very rigorously and accurately. Yet the only such application of the Lakatosian methodology to economics was made by Latsis,¹⁰ who developed a schema for the neoclassical research program. In the eyes of Latsis, the fundamental aspect of neoclassical theory is its situational determinism, a subject which broaches the psychological dimension of economic theory.

Economic Psychology

Neoclassical economics is often criticized for overemphasizing the individual economic agent at the expense of his context--the society around him. This is the starting point of a number of critiques of the neoclassical method. The general equilibrium system is seen to consist of many utility-maximizing individuals reposing in a universal general equilibrium, in a system which completely ignores the social environment, the institutional framework and historical background. Now the accusation of individualism is superficially similar to the

accusation that neoclassical theory is overly psychological. In other words, if economic theory is so intent on analyzing the individual as to ignore the environmental noise, then it is bound to emphasize the individual's cognitive processes, hence laying itself upon the accusation of being too psychological. It may come as a surprise, therefore, that some writers in this area have found the psychological content of neoclassical theory to be quite trivial. The level of psychological intricacy in the typical neoclassical problem has been compared to the following:

Predict the behavior of a driver whose automobile is traveling on a dry road at a speed of 60 m.p.h. at a distance of 10 feet behind another car, when the latter stops abruptly.

One safe prediction would be that the driver will slam on his brakes as hard and as quickly as he can. One could devise very elaborate psychological explanations of this prediction, but the prediction itself is not only safe but is also neutral with respect to the various psychological theories which could be brought to bear upon the problem. More formally, we can classify the problem as one in which the logic of the situation tolerates a unique solution, making a psychological study of the matter redundant. If the prediction failed to acquire corroboration, the predictor is likely to protest that significant data, such as suicidal tendencies on the part of the driver, have been held back.

Latsis finds the same to be true in the neoclassical research program in economics. Take for example the perfectly competitive model, where the seller is faced with a horizontal demand tangent to the long-run average cost curve at its lowest point. In this situation, the seller has no choice but to produce the quantity corresponding to the tangency. It is not just a matter of optimization; it is really a

question of survival. The market pushes the price level to a tangency with the average cost curve and the firm will go out of business unless its production takes place at the quantity corresponding to the tangency. Like the driver in the previous paragraph, the perfect competitor really has no choice. Latsis argues that in economics, problems are set up in such a way that the situation has only a single exit. Latsis emphasizes the link between the character of such solutions to the quality of psychological or socio-psychological content; yet one could equally emphasize the lack of a sociological or political content, which like the absence of a non-trivial psychological dimension, leads to the situational determinism.

To elaborate on the psychological aspects of this theme, I shall introduce Simon's distinction between substantive and procedural rationality,¹¹ a distinction which shall be needed again in our discussion of the rational expectations hypothesis in Chapter 4. Substantive rationality "depends upon the actor in only one respect--his goals. Given these goals, rational behavior is determined entirely by the characteristics of the environment in which it takes place." In the example of the driver, above, or the firm in the perfectly competitive model, the problem is set up in such a way that, given a goal--survival--the behavior of the actor is readily predictable, since the logic of the situation tolerates only one outcome. Rationality enters only in the determination of the goals, but has nothing to do with the procedure of achieving the goal. The situational assumptions determine uniquely the course of action which will lead the agent to that goal. Psychologists, however, usually have something different in mind when they deal with rationality, namely the procedural form of rationality. On procedural

rationality, Simon writes:

The process of rational calculation is only interesting when it is non-trivial--that is, when the substantively rational response to a situation is not instantly obvious. If you put a quarter and a dime before a subject and tell him that he may have either one, but not both, it is easy to predict which he will choose, but not easy to learn anything about his cognitive processes. Hence procedural rationality is usually studied in problem situations--situations in which the subject must gather information of various kinds and process it in different ways in order to arrive at a reasonable course of action, a solution to the problem.¹²

Hence, "behavior is procedurally rational when it is the outcome of appropriate deliberation." When a trade union is about to negotiate a wage contract, it faces the task of devising a procedure for figuring out what the rate of inflation is likely to be over the duration of the contract. Such problems in procedural rationality are much harder to handle than those dealing with substantive rationality, where, for example, the theorist assumes that the rate of inflation expected by the trade union is the same as that predicted by the model. More on this particular example in Chapter 4.

The distinction between the two forms of rationality is perhaps clarified by this quotation from Simon's explanation of an experiment by Feldman¹³

Suppose that you present a subject with a random sequence of X's and O's, of which 70% are X's and 30% O's. You ask the subject to predict the next symbol, rewarding him for the number of correct predictions. "Obviously" the rational behavior is always to predict X. This is what subjects almost never do. Instead, they act as though the sequence were patterned, not random, and guess by trying to extrapolate the pattern. This kind of guessing will lead X to be guessed in proportion to the frequency with which it occurs in the sequence. As a result, the sequence of guesses has about the same statistical properties as the original sequence, but the prediction accuracy is lower than if X had been predicted each time (58% instead of 70%).

In this case the study (typical of microeconomics) of what the subjects

rarely if ever do assumes substantive rationality, while an analysis of how the subjects attempt to outguess the pattern of X's and O's is a study in procedural rationality.

Simon's distinction between the various forms of rationality is not the first in the social sciences. Earlier, Weber distinguished between formal and substantive rationality. The extent of quantitative calculation, and the degree to which such calculation is then acted upon in the pursuit of, say, profit maximization, reflects the formal rationality of economic action. Monetization of the economy is necessary for formal rationality, which becomes manifested in accounting and budgetary management techniques. In contrast, substantive rationality is manifested in the "extent to which it is possible to secure what, according to a given system of values, is an adequate provision of a population with goods and services, and in the process remain in accord with the ethical requirements of the system of norms."¹⁴ In Weber's words, "it is not sufficient to consider only the purely formal fact that calculations are being made on grounds of expediency by the methods which are, among those available, technically the most nearly adequate. In addition, it is necessary to take account of the fact that economic activity is oriented to ultimate ends of some kind, whether they be ethical, political, utilitarian. . . ."¹⁵ Weber's distinction between forms of rationality manifested by the economy leads to the study of the potential for conflict between them. Unlike Weber's, Simon's distinction is between forms of rationality manifested at the same level of the economy (primarily microeconomic), and there is no hint of any hierarchy of types of rationality. The conflict in Simon's case is mostly in the mind of the methodologist and the theorist: should the latter set up

the model in a manner which endows impulsive action with its own form of rationality (since impulsive action will generate the right result, if constrained by the proper kind of situational logic), or should the theorist abstain from modeling this substantive form of rationality, and deal instead with problems where impulsive action becomes procedurally irrational?

Latsis' SRP

In the neoclassical model, the economy is sterilized not only of all extra-economic, social detail but also of most psychological intricacy. The absence of these qualities is borne out in Latsis' suggested neoclassical research program. Latsis, in his formulation of a neoclassical SRP (page 9), suggests the following propositions as comprising the hard core¹⁶:

- (i) Decision-makers have correct knowledge of the relevant features of their economic situation.
- (ii) Decision-makers prefer the best available alternative given their knowledge of the situation and of the means at their disposal.
- (iii) Given (i) and (ii), situations generate their internal 'logic' and decision-makers act appropriately to the logic of their situation.
- (iv) Economic units and structures display stable, coordinated behavior.

His version of the positive heuristic is as follows:

- (a) Construct static models.
- (b) Minimize and if possible completely eliminate psychological and, in general, non-economic content from the model.
- (c) Set up the situational assumptions in such a way that a determinate equilibrium issues. (Set up "Single-exit" situational models.)
- (d) Where possible construct functions which are suitable for the application of the procedures of the calculus.
- (e) If the model yields no determinate equilibrium, modify the situational assumptions until such a solution becomes possible.
- (f) When the model yields a determinate equilibrium, attempt to refine it by introducing more realistic situational assumptions.

The situational determinism of neoclassical theory is particularly evident in hard core proposition (iii) and positive heuristic instructions (b) and (c). For example, as applied to indifference curve analysis in consumer theory, hard core postulate (ii) translates into the assumption of utility maximization, while instruction (c) calls for the addition of such assumptions as non-satiation, convexity, and transitivity of preferences--in effect ensuring a unique solution (a "single exit") to the utility maximization problem.

One problem inherent in the Lakatosian methodology, is the sweep of the propositions in the hard-core. For example, it is conceivable that a weak version of Keynesian theory would qualify in the neoclassical program, if the latter's hard-core is watered down sufficiently. In the practical application of Lakatos' ideas, the scope of a program depends crucially on the inclusiveness of the hard-core, which could in turn be influenced by the vantage-point of the methodologist. For example, it could conceivably be argued (though Chapter 4 below reaches a very different conclusion) that monetarism is part of a wider neoclassical research program, which would itself compete with a Keynesian program. This extended neoclassical program would encompass a range of theories which generally make policy recommendations whose political flavor is usually labelled as conservative. However, this revised, neoclassical hard-core, besides being different from that of Latsis, would then also be different from what a methodologist would have constructed, say, 30 years ago, before the emergence of monetarism.

A case will be made in Chapter 3 for a separate, competing Keynesian program, as opposed to a mere Keynesian theory within the larger

neoclassical program--the justification being that Keynesian policy recommendations often vary widely from those of a neoclassicist. The positive heuristic of the Keynesian program directs the theorist to construct macroeconomic models, and as shall be argued in Chapter 3, this has a lot to do with the types of policy recommendations which emerge. This suggests the proposition (d) in Latsis' neoclassical positive heuristic above ought perhaps to be reworded to specify the microeconomic character of the static models. As it stands, the term "decision-makers" in the hard-core could possibly refer to macroeconomic policy makers, and this possibility must be excluded.

While an improvement over Kuhn's in that it allows for the coexistence of rival, powerful programs, Lakatos' framework shares with Kuhn's a suggestion of the social setting of science. The influence of the intellectual environment on the individual scientist is manifested, in particular, in Lakatos' negative heuristic. This context-dependency of economic thought is further explored in the next section.

The Role of Econometrics

Some methodologists believe that change in economic theory is the result of hard thinking by academicians and ruthless testing by econometricians. In contrast to this objectivist attitude, relativists view the social environment of economists as the major determinant of economic thought.¹⁷ The true picture probably lies somewhere between the two positions. For one thing, crucial testing often comes from, and therefore varies with contemporary events and developments in society and in these situations there is an element of truth in both points of view. For example, the early Keynesian models, which ignored the price level

and the rate of inflation, were widely acceptable in the inflation-free 50's, but were less and less satisfactory as inflation accelerated in later decades. The objectivist would hold that Keynesian theory failed to explain the course of price movements and was therefore refuted. The relativist would counter that such a refutation, aside from its correctness or otherwise, is itself a function of the times and their problems: were not inflation such a problem in the '70's, this refutation would never have come about and Keynesianism would now be more widely adhered to. There seems to be a social process of evidence-selection, where only certain aspects of the total evidence are investigated. (To take another example, unemployment constituted a social and economic problem well before Keynesian times, but it was not fully recognized as such.¹⁸) What is being suggested is that in the same manner as the theorist selects out certain variables and certain relationships for his detailed study, so also the econometrician is likely to analyze not the entire span of human history, but merely a short part thereof. To come back to the question of inflation in Keynesian theory, one should mention that some objectivists may appropriately add that the inflation of the '70's is itself the result of the earlier, misguided policies suggested by Keynesian theory, and that these recommendations simply failed the test of the '70's.

Here lies another crucial characteristic of the social sciences. The experiment of the natural scientist usually leaves the subject matter unchanged, but the theorizing of social scientists, especially when buttressed by the evidence, may lead to policy changes, which in turn--through their effect on economic or other variables--will influence the nature of the evidence as received by future social scientists.

Clearly, econometricians who respect their own profession would adhere to the objectivist position, which is in turn part of the wider positivist view. For a positivist, econometrics play an important role in the search for regularities in the data, and in projecting these regularities into the future. Degeneration or progression of the SRP depends crucially, by this view, on the econometric results. (Others, however, are dubious of the ability of econometrics to discriminate between good and bad theories and they point to the endless debates in the literature.)

The positivist approach seeks to expand the predictive capabilities of economics: true science enables us to predict future experience, and so control our environment.¹⁹ The positivist procedure is to set up models whose predictions are then tested. The "problem of induction" recognizes that it is impossible for a test or any number of tests to verify a universal statement, or even of negating it. Just because the sun has so far risen every morning, it does not follow that the universal statement, "the sun rises every morning" or "the earth turns every day," is true. Also, the negation of a universal statement by some crucial experiment can be circumscribed quite easily. For example, the typical test involves not one hypothesis in isolation, but rather a whole set of hypotheses. If the results are negative, the whole set is refuted, but this does not necessarily mean that each hypothesis is false. So, if I test the statement, "Quantity demanded depends upon the price, all other things being equal," and the test proves negative, I can, if I so choose, maintain that quantity demanded does indeed depend upon the price, but that during the test, one or more of the "other things" changed. The crucial experiment, the one

which makes or breaks the theory, so to speak, is very rare and so the tester can usually only collect evidence for, or against a hypothesis, in other words, resort to probabilistic corroboration. "To say that an observation increases the probability of a hypothesis . . . is equivalent to saying that the observation increases the degree of confidence with which it is rational to entertain the hypothesis."^{20,21}

This approach, with all its limitations, is common to all the sciences, whether natural or social. The best that can be achieved in any science is not absolute verification, but merely probabilistic corroboration: the econometrician, for example, never seeks to prove a hypothesis, but merely to gather further supporting observations, enhancing the probability that a hypothesis is correct. It is in the very nature of this method that the interpretation of the statistical results is somewhat subjective, partly because no test is powerful enough to yield clear and overriding results. To take the simplest of examples, a comparison of the correlation coefficients for two separate, single equation models (with the same dependent variable) is valid only when they have a common number of independent variables. Even if this condition is met, other test statistics (such as the t-statistics corresponding to the parameter estimates) need to be consulted, and there is no assurance that different statistics will yield converging results. The problems of interpretation become more intricate as the number of equations increases.

These difficulties are compounded by the peculiarities of the social sciences. Some economists scoff at the differences between the natural and the social sciences and point out that astronomers too cannot conduct experiments with the planets and the stars, and that therefore

there is nothing intrinsically different between the non-laboratory method of economics and the method of the natural sciences. This observation depends crucially on the interpretation of the word "intrinsically." Natural scientists generally conduct their experiments under conditions which allow them to maintain total control over which variables participate--human genetics and astronomy being major exceptions. The typical problems of econometrics--omission of a relevant variable from an equation, the inclusion of an irrelevant variable, serial correlation, simultaneity bias, heteroscedasticity, and all the complications manifested in the behavior of the error term--are of little concern to the natural sciences, where the experimental method and the process of Fisherian randomization ensure that the error term is harmless.²²

The problems discussed in econometric theory deal with instances where the assumptions underlying the classical linear regression model (CLR) are not satisfied. With all the assumptions met, the ordinary least squares method would yield parameter estimates from which information about population parameters can readily be inferred. If, for example, the relationship between the variables under study is not linear, a crucial CLR assumption is breached. The problem of non-linearity is obviously not unique to econometrics or any other non-experimental science. (Of course, in the non-experimental sciences there is a greater likelihood--because of the difficulties inherent in the testing procedures--that the scientist will be compelled to extrapolate his results, valid as they may be for small ranges where linearity is a reasonable approximation, to a longer span where non-linearities come into play.) But the majority of the problems are due entirely

to the non-experimental nature of econometrics. Take the problem of multicollinearity--the breach of the CLR condition that no pair of independent variables be linearly related. If the economist could construct his own experiments and rigidly control the variables, the problem of multicollinearity would more easily be controlled. As important are the problems relating to the error term--the non-zero mean of the disturbance term, heteroscedasticity (or non-constant variance of the error term), autocorrelated error term and so on. It is only when research is carried on outside the laboratory that the error term becomes very troublesome and such problems assume significant proportions.

Another problem which has nothing to do with the breach of any CLR assumptions, but still reflects the difficulties of the non-experimental method is that of deriving parameter estimates for the original equations in a simultaneous system after the reduced forms are put to the test, namely the problem of identification. Complete control over the variables, as in the experimental sciences, excludes the overlap between endogenous and independent variables: in an experimental test, independent variables are more strictly controlled by the scientist.²³

The presumption that the methods of the natural sciences carry over well to the social sciences can perhaps be challenged from a different angle, which has to do with the multi-level nature of causality in social systems. Suppose one were to attempt a mathematical representation of a situation where the rate of inflation is perceived as being positively related to trade union pressure on wages, accommodated by an expansive monetary policy. The permissiveness of monetary policy is a condition necessary in the model for trade union pressure to be transmitted into higher prices. A regression one could test in an attempt

at empirical corroboration would be:

$$P = a_0 + a_1 T + a_2 M,$$

where T is a proxy for trade union strength, M stands for monetary growth, P for the rate of inflation. Then a_1 is equal to the partial derivative, $\frac{\partial P}{\partial T}$, which measures the influence of T on P, with an unchanging M. Now suppose the model accurately represents reality, that is that trade unions do indeed cause inflation, but only if the monetary authorities allow them to. In that case, the partial derivative of P with respect to T will equal zero: without a monetary change, trade union cannot influence the rate of inflation. But such a result in no way justifies the statement that "trade union power has no effect on inflation." On the contrary, trade unions serve as a conduit for monetary pressures on the rate of inflation. The trouble is the contribution of trade unions is different from that of the monetary authorities: the two influences do not operate at the same level. We have a hierarchy of influences, while econometrics appears to capture a relationship at only one level of the hierarchy.²⁴

The intrinsic limitations of econometric tools reinforces the importance of the a priori. A pertinent episode is recounted by H.O. Wold,²⁵ who notes that in the experimental sciences, the estimation of a_1 and b_1 in

$$y = a_1 + b_1 z$$

and the estimation of a_2 and b_2 in

$$z = a_2 + b_2 y$$

is such that

$$a_2 = -\frac{a_1}{b_1} \text{ and } b_2 = \frac{1}{b_1}$$

The problem of the "choice or regression" does not arise in the experimental sciences, since the researcher can easily keep track of which one was the independent variable, and can therefore compute the parameter estimates accordingly. With the non-experimental method, however, the two regression relationships become

$$E(y|z) = a'_1 + b'_1 z$$

and

$$E(z|y) = a'_2 + b'_2 y$$

The presence of an error term in the computation implies that

$$b'_2 \neq \frac{1}{b'_1} \text{ and } a'_2 \neq -\frac{a'_1}{b'_1}$$

In 1906, P. Mackeprang attempted to estimate the price elasticity of coffee and could not make up his mind between the two alternative estimates, 0.42 and 0.83, and presented the problem as involving a "choice of regression." Nowadays, most economists would choose the first estimate, obtained by regressing quantity on price. That such a choice would be made regardless of what the summary statistics indicate reflects the importance of the a priori and the weakness of econometric tools.

A combination of some of the problems alluded to above appears to be at work in the ongoing debate about the St. Louis equations. The

original tests published in 1968 by Andersen and Jordan²⁶ assessed the relative strength of monetary and fiscal policy (the former as measured by the narrowly defined money stock, the latter by high employment federal expenditures) and found the former to be more effective. A reply in 1969, by De Leeuw and Kalchbrenner,²⁷ disputed these findings on the grounds that the independent variables were not truly exogenous, a condition necessary for the absence of correlation between the independent variables and the error term. Benjamin Friedman²⁸ retested the equations with data through 1976 and found fiscal policy to be more effective: the St. Louis equation now believed in fiscal policy. Carlson²⁹ rejected Friedman's results: he found that heteroscedasticity plagued the Friedman data, but not the original Andersen-Jordan data. To guarantee homoscedasticity Carlson reestimated the Friedman equation: whereas Friedman (like Andersen-Jordan) used the first differences of all the variables, Carlson substituted rates-of-change. Vrooman³⁰ complained that this switch in specification destroys the comparability of the results with the original Andersen-Jordan, and also suggested that the observed heteroscedasticity reflects more than a statistical problem: the St. Louis equation might have been suffering from misspecification all along. Difficulty in resolving these issues seems to stem primarily from the non-experimental nature of econometrics manifested in a troublesome error term, and possibly also from the failure of single-level equation systems to handle structural shifts through time.

In closing, mention ought to be made of another possible source of weakness in econometric testing: the quality of the statistical data. It could be argued that the collection of data is itself subject to theoretical considerations. It is itself paradigmatic, or (in

Lakatosian terms) it belongs within a scientific research program and is therefore performed according to the rules of some positive heuristic. If this program is a larger one which also includes economic theory, there might be an automatic bias testing of such theory as well as theories in competing programs. This application of Lakatos' SRP to statistical data collection is not discussed in the literature, but there are many indications that it could be a fruitful line of research. A sampling of areas of potential problems would include: Von Mises³¹ categorically states that the insolubility of the index number problem implies the nonsensical nature of the concept of the absolute price level; present-day adherents to the Austrian school accordingly reject the social accounting systems of macroeconomics.³² Baumgartner and DeVille³³ raise serious questions about the disparity in the treatment in national income accounting between labor and capital. Harris³⁴ questions the usefulness of published macroeconomic data to Marxist analysis. Though it does not necessarily follow that this issue is relevant to the statistical analysis of the monetarist-Keynesian debate, it is deserving of further consideration.

Conclusion

Empirical testing is surely a healthy exercise which stimulates and enhances the development of economic theory. Yet it does have some limitations. And it is these limitations which lead to the predicament of some major economic debates. Wilber traces the development of the controversy surrounding the Friedman-Meiselman findings, in a manner similar to the discussion of the St. Louis equation above. He adds, "when the theory becomes so immune to refutation, it functions more as a prescriptive than a descriptive device. This is the way the Keynesian-

monetarist controversy should be seen."³⁵ According to the positivist methodology, of course, prescriptions, or normative statements, do not belong in scientific practice. With these thoughts in mind, the following chapters concentrate on the a priori in microeconomic and macroeconomic monetary theory.

Chapter Notes

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- ²M. Friedman, A theoretical framework for monetary analysis, National Bureau of Economic Research, Occasional Paper, New York: Columbia University Press, 1971, pp. 31-34.
- ³M. Friedman, "The methodology of positive economics," in Essays in positive economics, Chicago: University of Chicago Press, 1953.
- ⁴T. Kuhn, The structure of scientific revolutions, 2nd edition, Chicago: University of Chicago Press, 1970.
- ⁵K. R. Popper, Conjectures and refutations: the growth of scientific knowledge, New York: Harper and Row, 1963.
- ⁶T. S. Kuhn, The essential tension, Chicago: University of Chicago Press, 1977.
- ⁷This view is expressed by P. K. Feyerabend in "Consolation for the specialist" in I. Lakatos and A. Musgrave, Eds., Criticism and the growth of knowledge, Cambridge: Cambridge University Press, 1970.
- ⁸Applications of Kuhn's ideas to economics include D. F. Gordon, "The role of the history of economic thought in the understanding of modern economic theory," The American Economic Review, 1965, 55, pp. 119-127; A. W. Coats, "Is there a 'structure of scientific revolutions' in economics?," Kyklos, 1969, 22, pp. 289-296; R. D. C. Black, A. W. Coats and C. D. W. Goodwin, The marginal revolution in economics, interpretation and evaluation, Durham, N.C.: Duke University Press, 1973; B. Ward, What's wrong with economics?, New York: Basic Books, 1972; J. Baumberger, "No Kuhnian revolutions in economics" in Journal of Economic Issues, March 1977, 11, No. 1, pp. 1-20; D. Winch, Economics and policy, a historical study, New York: Walker, 1969, pp. 174-176.
- ⁹I. Lakatos, "Falsification and the methodology of scientific research programmes" in I. Lakatos and A. Musgrave, Eds., Criticism and the growth of knowledge, Cambridge: Cambridge University Press, 1970, pp. 91-195; "History of science and its rational reconstructions" in C. Howson, Ed., Method and appraisal in the physical sciences: the critical background to modern science, 1800-1905, Cambridge: Cambridge University Press, 1976, pp. 1-39.
- ¹⁰S. J. Latsis, "A research program in economics," in S. J. Latsis, Ed., Method and appraisal in economics, Cambridge: Cambridge University Press, 1976, pp. 1-41.

- 11H. A. Simon, "From substantive to procedural rationality" in S. J. Latsis, Ed., op. cit.
- 12H. A. Simon, ibid.
- 13H. A. Simon, op. cit.; J. Feldman, "Simulation of behavior in the binary choice experiment" in E. A. Feigenbaum and J. Feldman, Eds., Computers and thought, New York: McGraw-Hill, 1963.
- 14T. Parsons, introduction to M. Weber, The theory of social and Economic organization, New York: Free Press, 1947, p. 35.
- 15M. Weber, op. cit., p. 185.
- 16S. J. Latsis, ibid., p. 22.
- 17For a strong contrast between these positions, compare M. Blaug, Economic theory in retrospect (3rd edition), Cambridge: Cambridge University Press, 1978 with L. Rogin, The meaning and validity of economic theory, New York: Harper, 1956.
- 18See D. Winch, ibid., p. 167.
- 19A. J. Ayer, Language, truth and logic, New York: Dover Publications, 1952, p. 50.
- 20A. J. Ayer, op. cit., p. 101.
- 21In Lakatosian terminology, the failure to generate testable hypotheses and gather supporting observations leads to the degeneration of the scientific research program.
If one wants to replace the logical empiricism with a Popperian flavor, then the econometrician is said to actively seek to refute, probabilistically, his hypothesis.
- 22H. A. Wold, "Econometrics as pioneering in non-experimental model building," Econometrica, July 1969, 37, No. 3, pp. 369-381; T. Haavelmo, "The role of the econometrician in the advancement of economic theory," Econometrica, July 1958, 26, No. 3, pp. 351-357; H. Wold, "Causality and econometrics," Econometrica, 1954, 22, pp. 162-177.
In an ambivalent statement, which does not quite resolve this issue, Friedman argues, "evidence cast up by experience is abundant and frequently as conclusive as that from contrived experiments; thus the inability to conduct experiments is not a fundamental obstacle to testing hypotheses by the success of their predictions. But such evidence is far more difficult to interpret. It is frequently complex and always indirect and incomplete. . . . The denial to economics of the dramatic and direct evidence of the 'crucial'

experiment does not hinder the adequate testing of hypotheses; but this is much less significant than the difficulties it places in the way of achieving a reasonably prompt and wide consensus on the conclusions justified by the available evidence." (M. Friedman, "The methodology of positive economics," op. cit.)

²³Perhaps the most striking, recent example of the identification problem has to do with the Solow-Tobin test of the natural rate of unemployment. They estimated the equation

$$U_t = \beta_1(p_t - {}_t p_{t-1}^*) + \beta_2 {}_t p_{t-1}^* + \sum_i \lambda_i U_{t-i} + \epsilon \quad (1)$$

where U_t stands for the unemployment rate in period t , p_t is the rate of inflation at period t , and ${}_t p_{t-1}^*$ is the rate of inflation expected in period $t-1$, looking forward one period, that is to period t , and where

$${}_t p_{t-1}^* = \sum_i v_i p_{t-i} \quad (2)$$

From (1) and (2), it follows that

$$U_t = \beta_1 p_t + (\beta_2 - \beta_1) \sum_i v_i p_{t-i} + \sum_i \lambda_i U_{t-i} + \epsilon \quad (3)$$

Unfortunately, no test of (3) can ever generate an estimate of β_2 , and without such an estimate no light can be shed on the natural rate hypothesis. In other words, that parameter is unidentifiable from the estimates of equation (3). A way out is to assume that the sum of weights v_i is 1. But that presumes away the rational expectations hypothesis. This is an ugly dilemma for those proponents of the natural rate hypothesis who are also adherents to the rational expectations hypothesis. (See Thomas J. Sargent, "Testing for neutrality and rationality," in Federal Reserve Bank of Minneapolis, A prescription for monetary policy: proceedings from a seminar series, 1976.)

²⁴In dealing with this matter, Kennedy feels that this problem is quite easily resolved.

It could be that β is itself determined by variables outside the model. For example, the extent to which a firm reacts to demand changes may depend on government policy parameters such as tax rates. This problem is most easily resolved by substituting the relationship determining β directly into the original estimating function. Thus if we have for example,

$$y = \beta_1 + \beta_2 x_2 + \epsilon$$

and β_2 , say, is determined as

$$\beta_2 = \alpha_1 + \alpha_2 z_2$$

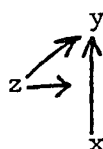
we can combine these relationships to get

$$y = \beta_1 + \alpha_1 x_1 + \alpha_2 (x_2 z_2) + \varepsilon$$

so that estimation should be undertaken by including the new variable $(x_2 z_2)$ as an additional regressor.

(P. Kennedy, A guide to econometrics, Cambridge: MIT Press, 1979.)

Aside from the question as to whether one is prepared to linearize the determination of β_2 , there is a further problem with Kennedy's suggestion. Suppose one wishes to test the hypothesis that monetary policy (z) influences nominal income (y) via two transmission channels. One operates directly on y , the other operates indirectly through fiscal policy (x). This means that not only does monetary policy directly stimulate income (through, say, the real balance effect), but also through its accommodation of fiscal policy. Let the arrows represent direction of influence; then



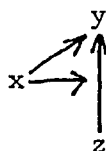
Then assuming linear relationships throughout, we follow Kennedy's suggestion, and end up with

$$y = a_0 + a_1 x + a_2 z$$

$$a_1 = b_0 + b_1 z$$

$$\text{Therefore } y = a_0 + b_0 x + b_1 x z + a_2 z \quad (1)$$

Now let us set up, as a mental experiment, the following alternative system:



which is a radically different version of the model: it suggests that fiscal policy affects income directly as well as indirectly, through monetary changes (the roles of monetary and fiscal policy are in a sense reversed). Then,

$$y = a_0 + a_1 z + a_2 x$$

$$a_1 = b_0 + b_1 x$$

$$\text{and therefore } y = a_0 + b_0 z + b_1 x z + a_2 x \quad (2)$$

Equation (2) is not identifiable from equation (1). This seems to be a problem which often follows when multi-level systems are collapsed into single-level systems.

- 25H. O. Wold, "Econometrics as pioneering in nonexperimental model building" in Econometrica, July 1969, 37, No. 3, pp. 369-381.
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- 27F. DeLeeuw and J. Kalchbrenner, "Comment," Federal Reserve Bank of St. Louis, Review, April 1969, pp. 6-11.
- 28B. M. Friedman, "Even the St. Louis model now believes in fiscal policy" in Journal of Money, Credit and Banking, May 1977, 9, pp. 365-367.
- 29K. M. Carlson, "Does the St. Louis equation now believe in fiscal policy?," Federal Reserve Bank of St. Louis, Review, February 1978, pp. 13-19.
- 30J. Vrooman, "Does the St. Louis equation even believe in itself?," Journal of Money, Credit and Banking, February 1979, 11, No. 1, pp. 111-117.
- 31J. Von Mises, The theory of money and credit, Translation by M. E. Batson, New Haven: Yale University Press, 1953, Part One, Chapter 5.
- 32L. M. Lachmann, "Towards a critique of macroeconomics," in E. G. Dolan, Ed., The foundations of modern Austrian economics, Kansas City: Sheed and Ward, 1976.
- 33T. Baumgartner and P. DeVillè, "National income accounts and economic crises," Contradictions, June 1978, No. 15-16, pp. 75-85.
- 34L. Harris, "On interest, credit and capital," Economy and Society, May 1976, 5, No. 2, pp. 145-177, especially section 3.
- 35C. K. Wilber, "Empirical verification and theory selection: the Keynesian-monetarist debate," Journal of Economic Issues, December 1979, 13, No. 4, pp. 973-982.

CHAPTER 2

A NEOCLASSICAL MONETARY MODEL

The previous chapter introduced Lakatos' scientific research program (SRP), and its application to economics by Latsis. The latter dealt mainly with the theory of the firm in various market structures--theories which form part of the larger neoclassical SRP. Other areas of neoclassical theory which could be presented in a similar format would include consumer and growth theory, as well as monetary theory. In each, the distinguishing criterion--the characteristic which separates them from theories belonging to competing programs--is the microeconomic situational determinism (page 10) built into the models. This determinism in turn is predicated on the presumption of the substantive form of rationality. This is particularly relevant for the purposes of this dissertation because this determinism is shared by the rational expectations hypothesis, a subject of discussion in Chapter 4.

This chapter looks at neoclassical, microeconomic, monetary theory in some detail, in preparation for the discussion of Keynesian and monetarist theory in later chapters. It will suggest a qualification to the assertion that neoclassical monetary theory is situationally determinate in all its aspects.

The Neoclassical Dichotomy¹

The term "transmission mechanism" refers to the manner in which monetary changes, originating in policy or otherwise, affect economic

variables. The centerpiece of neoclassical monetary theory is the so-called neutral nature of money. Neutrality means that any effect of monetary changes on variables other than the absolute price level is temporary, to be reversed in a long run where all quantities and relative prices return to their starting position. This chapter will address itself to those transmission channels whose influence is restricted to the absolute price level.

Despite the agreement among such neoclassical models on long-run neutrality, there has been substantial controversy as to the nature and specific formulation of a model which, while satisfying all the neoclassical criteria, will not, at the same time, suffer from logical flaws. Logical elegance and consistency are among the hallmarks of the neoclassical method;² hence, the search for logical contradictions in any model which claims to be neoclassical.

All neoclassical models separate the determination of real variables from that of nominal values. In some simplified models this is achieved by having a set of equations determine the real variables, namely the level of output and relative prices. One additional equation introduces the money supply and sets the absolute price level. The "real" set is self-contained and has as many equations as unknown variables. The dichotomy of monetary from value theory is complete.³ Consider one such macroeconomic model:

$$\begin{aligned} s &= i & (1) \\ s &= s(r) & (2) \\ i &= i(r) & (3) \\ q &= q(L) & (4) \\ L &= L(W/P) & (5) \\ W/P &= q_L(L) & (6) \end{aligned}$$

In this model, six equations determine six unknowns: q (output) and its two components, s (saving) and i (investment); two relative prices, r

(the interest rate) and W/P (the real wage); and L (the labor force). The addition of a further equation--the quantity equation--

$$M = q PK \quad (7)$$

determines a further unknown, P (the absolute price level); M is the money stock and K is a constant.

This model, although aggregative, has traditional, neoclassical leanings. Although it borrows from the Keynesian method in the use of such aggregate concepts as saving and investment, several features distinguish it from the typical Keynesian models. For example, the labor market (equations (5) and (6)) take the economy to full employment, and this level of employment is then related to the full-employment level of output (equation (4)). The equality of aggregate demand and aggregate supply therefore occurs automatically at full employment.

A closer look reveals a possible problem in the model. We first introduce Walras' and Say's Laws. The former states that any system of markets taken as a whole will have a net excess demand equivalent to zero. Excess demand in one or more markets must necessarily have a counterpart excess supply in one or more other markets, for a net excess demand of zero. The logic behind this is that the offer, or supply, of goods on a market is only one facet of a market transaction or an attempt at a market transaction; other goods, or more likely money, are expected and demanded in return. The implication is that Walras' Law is an identity and will hold at all times, both in and out of equilibrium.

But is the same true of the markets for commodities taken on their own, in other words the markets for all goods and services except money? Say's Law answers this question in the positive: there can be no net excess demand in all commodity markets taken together. Let us consider

the identity version of Say's Law and examine the only two possible eventualities: Say's Identity is either true or untrue.

Suppose first that Say's Identity is true, and let us analyze the consequences for a monetized economy. This identity implies that excess demand for the real sector as a whole is identically (that is, irrespective of whether the real sector is in equilibrium or not) equal to zero. Invoking Walras' Law, we can adduce that there can be no excess demand, or disequilibrium, in the money market. Money supply and money demand are identically equal. If the equality in (7) above were replaced by an identity sign ($=$), P would be rigidly defined as M/qK , whereas an equation would have told us about the behavior of P when in equilibrium, and would have then set us on the separate task of finding P 's behavior out of equilibrium. A definition cannot indicate any causality. As a result, P could change and since q is exogenous for the purposes of the equation, M would simultaneously follow suit and change by the same proportion. In that sense, P is indeterminate. This unsatisfactory state of affairs can be corrected by reneging on Say's Identity.

Therefore suppose that Say's Identity is not true. Then, a situation might occur where the real sector is in disequilibrium and, in view of the dichotomy (the complete separation of money from real markets), the money market will search independently for its own equilibrium. In this situation, it is entirely possible that the commodity markets be in disequilibrium while the money market reaches a state of equilibrium. The dichotomy removes all assurance that Walras' Law--that the sum of excess demands in all markets be equal to zero--would hold. The disconnection of the monetary sector from the rest of the economy removes the mechanism which would have insured the validity of Walras' Law. This

is an equally undesirable situation.

The only way out of this bind is to breach the dichotomy, and include money in the real sector. This can be achieved by including money inside the utility functions. (These functions would be included in the general equilibrium model which underlies the aggregative model.) Then we would reject Say's Identity, and include money in the real sector. The interrelationship between the money and the commodity markets would ensure that, when the commodity market is in disequilibrium, so is the money market. Net excess demand in either one will be accompanied by net excess supply in the other, for a total, economy-wide net excess demand of zero, thus ensuring compliance with Walras' Law.

We can refer to the absolute price level as the relative price of nominal money. In an aggregative model, with one composite commodity,⁴ the relative price of nominal money stands for the amount of the composite commodity that will exchange for one unit of nominal money. (In a more decomposed, microeconomic model, the absolute price level is replaced by the set of relative prices of nominal money for each of the various commodities.) An increase in the supply of nominal money can be expected to have repercussions on this relative price. There can be no monetary effect on the rest of the economy (i.e., there can be no monetary transmission mechanism) unless the model integrates the monetary and commodity sectors. A dichotomy of the two would make the demand for commodities functionally independent of the holdings of money; this would leave the rate of exchange of money for commodities totally indeterminate. Foregoing the dichotomy permits monetary changes to affect the markets for commodities; it is only by virtue of this effect that monetary changes can influence the absolute price level.

The real balance effect, made possible by the inclusion of M inside the utility function, constitutes the transmission channel in neoclassical theory.

The Real Balance Effect⁵

Inclusion of money in the utility function implies that the utility maximizer will allocate his resources between additions to his money stock and the purchase of commodities. The utility maximizer will begin the exchange period with initial endowments of commodities and money. In figure 1, real balances are represented on the vertical axis, real income on the horizontal axis. If real income or output at the beginning of the period were OA and real balances AF , the budget line would be CD , which has a slope of 135° . Although the budget line reaches point C on the horizontal axis, real output cannot exceed OA , which represents income at full employment. An individual can always exchange output for real balances, but the representative individual cannot obtain output in excess of OA .

Take initial point F , the intersection of AZ (the perpendicular at real income A) with the budget line CD . Let F be off the expansion path OE . In such an event, the utility maximizer will reduce his real balances by FH --the vertical distance between F and R --in order to consume OG in commodities. As a result, real balances will shrink to AH by the start of the "next" period, and when the "next" period comes around the utility maximizer will move along his new budget line to J , which lies on the expansion path. Again, real balances decline to AK by the next period. This process goes on until M is reached. Here the utility maximizer is in long-run equilibrium, defined along long-run Hicksian

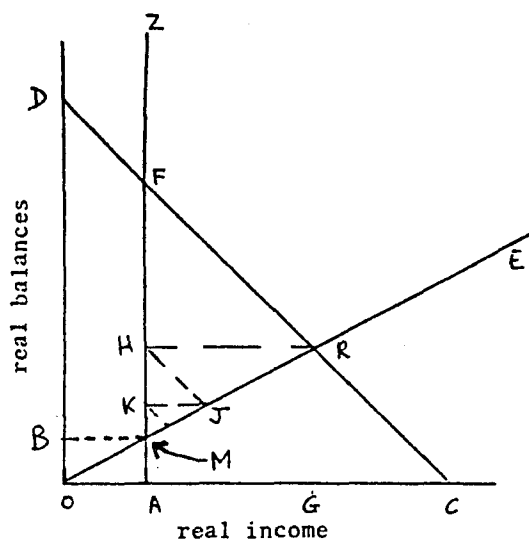


FIGURE 1

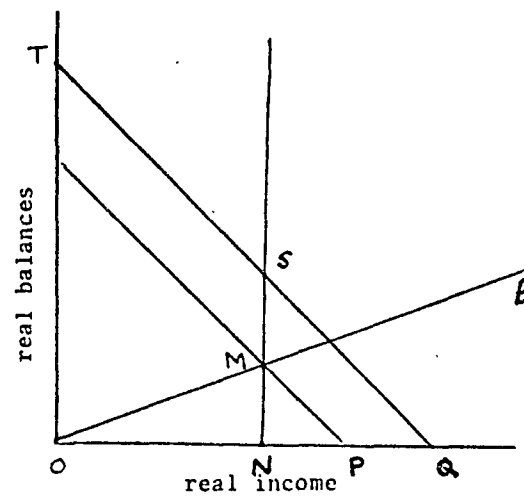


FIGURE 2

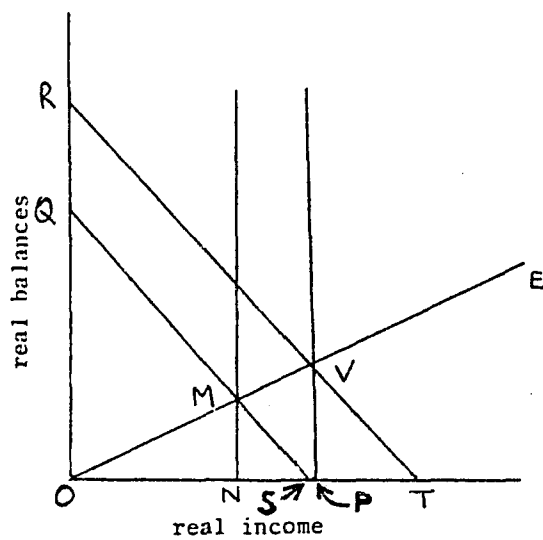


FIGURE 3

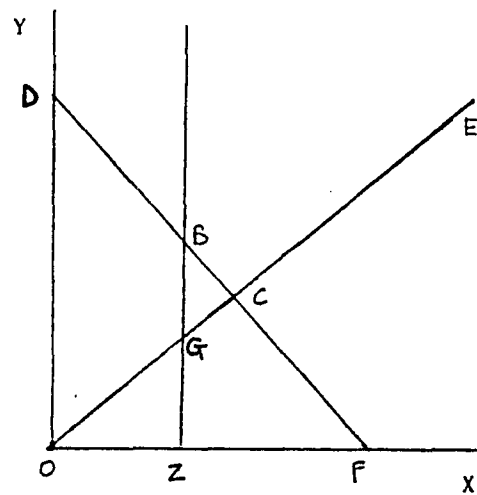


FIGURE 4

lines. Hicksian full equilibrium occurs "not merely when demands equal supplies at the currently established prices," that is at such points as R, H, J, and so on, but "also when the same prices continue to rule at all dates." The latter situation, at point M, represents equilibrium over time, or long-run equilibrium, and will be the only position sanctioned by the Walrasian auctioneer.⁶ From that point onwards, Walrasian general equilibrium is perpetually maintained, barring some change in some exogenous variable: the real money stock does not change from this point onwards. Note that changes in the stock of real balances would imply excess supply or demand for commodities, which cannot be allowed in long-run equilibrium.

Consider now, in Figure 2, an increase in real balances from an initial position of equilibrium at M. Let real balances grow from NM to NS. The budget line will shift to QT and the utility maximizer will find himself at S, the intersection of the new budget line with the perpendicular at the unchanging real income level, N. The same mechanism described in the previous paragraph will drive the utility maximizer to M.

On the other hand, an increase in real income from ON to OP (in figure 3) will shift the budget line from QS to RT, with the eventual new equilibrium moving from M on the old budget line to V on the new.

Implicit in this approach is the assumption that changes along the vertical axis, which represents real balances, will in no way influence relative prices or the quantity of the composite commodity represented on the horizontal axis. Note also that the ultimate, equilibrium position along the budget line is defined and completely determined by the level of real commodity output and the expansion path. Although desired money holdings are recognized as being determined by the choice

criteria of utility maximizers (as described along the expansion path), the monetary sector could justifiably be called a passive appendage to the real (commodity) sector. Production and exchange conditions in the commodity sector determine the real quantity of the composite commodity and the relative price structure within that composite commodity. With the utility map determining the expansion path, the quantity of real balances is endogenous and will change in relation to the quantity of real output, in a relationship defined by the expansion path. Monetary policy as we understand the term, is neutral and insignificant. Any change in the money supply has no effect on relative prices, real output or total utility. There is therefore a transmission mechanism, but its effect is purely on an irrelevant variable, the absolute price level, which does not enter the utility function. The only relevant monetary variable is determined endogenously, not by the money supplier.

These results emerge from the nature of a utility function, which bears closer examination. The utility function is homogeneous of degree zero in the nominal money stock and all prices⁷:

$$U = U(q_1, q_2, \dots q_n; M, p_1, p_2, \dots p_n)$$

If all the variables to the right of the semi-colon were to double, for example, total utility would remain unchanged. As resources are fully employed and the economy is satiated with real money balances, total utility is taken to its highest possible level U^* . Monetary satiation is assured by the homogeneity postulate: changes in absolute prices will take real money balances to their desired level, without having any other repercussions on the economy. With U as U^* , any changes in M , given the homogeneity postulate will require proportionately equivalent changes in all prices for equilibrium to be restored. The

homogeneity postulate is the reason for the neutrality of money.

The statement "money is neutral" means that, following a change in the money supply, the exchange rate between any pair of commodities will return to its original position by the time the system returns to an equilibrium position. In other words, the relative price of nominal money in terms of each of the various commodities will change by the same proportion. We can restate the above as follows:

$$\partial P_i / P_i = \partial M / M \quad \text{for any } i,$$

$$\text{and } \partial (P_i / P_j) / \partial M = 0 \quad \text{for any } i, j,$$

which will hold only if

$$\partial (U_i / U_j) / \partial M = 0 \quad \text{for any } i, j.$$

where i, j are commodities, M is money and U is the social utility function.⁸

Situational Determinism and the Money Supply

There can be little doubt that this monetary model shares all the propositions of the hard-core of neoclassical research program reproduced from Latsis in Chapter 1 and also observes all the instructions in the positive heuristic of that program. Proposition (i) dealing with the adequacy of knowledge and proposition (ii) concerning optimization are both met in the monetary model. Situational determinism--the essence of proposition (iii) of the hard-core, and also required by instruction (c) of the positive-heuristic--is very evident too: the path to long-run equilibrium at the intersection of the real income line and the expansion path is inevitable and obvious. The model is static [positive heuristic(a)], amenable to the calculus [positive heuristic (d)], and clean of absolutely all non-economic content [positive

heuristic(b)]].

The previous paragraph addressed itself to various aspects of monetary demand and monetary equilibrium. It may be fruitful to reevaluate the neoclassical situational determinism from a different angle: how does it manifest itself with regard to the money supply? To rephrase this question, how does money compare with an ordinary commodity? The supply of the latter in the neoclassical theory of the firm is characterized by situational determinism. Can the same be said of the money stock?

Viewed as a real quantity, money in the neoclassical model is similar, in many respects, to any other good. For example, an increase in the money supply from a situation of equilibrium, where all exogenous variables and parameters remain unchanged, will eventually totally reverse itself. An increase in the money stock which represents, say, an experiment on the part of the central bank will temporarily constitute an increase in the real stock of money; but since the system was at rest to start with, and because there was no change in any of the exogenous variables or parameters, the system will eventually return to the status quo ante, with the real money stock back at its original level. There is here a close parallel with what would happen in a perfectly competitive economy where producers of some commodity, although already maximizing their profits, decided as an experiment to produce more of their product. The excess supply would depress the price sufficiently for all of their output to be sold, but the new price would be uneconomic enough to induce the sellers to cut production back to its original level; the relative price would follow suit. Similarly, an increase in the real money stock will reverse itself, and the only difference

between the old and the new equilibrium will be in the absolute price level.

This analogy breaks down, however, on closer investigation. In the real world, market forces do not usually penalize the producers of money for their additional output. One also notes that, while the producers of the commodity unilaterally reduced their output after finding themselves unable to sell their higher level of output at the original price, the same is not true of money producers; it is not by way of any act of money producers that the money stock returns to its original level.

Note also that in the commodity example, neoclassical theory predicts that the new equilibrium price will be identical to that at the old equilibrium. So to carry on with the analogy we have to identify the relative price of real money. By analogy with the commodity industry, we would expect the relative price of real money to return to its old level at the new equilibrium. Here we run into conceptual problems. Real money has by its very definition a fixed rate of exchange. A dollar at 1956 prices is always what a dollar would have bought in 1956. The analogy with the commodity industry collapses once we recognize that, while the relative price of a commodity will change during disequilibrium and finally return to its original level, in the case of the money industry the relative price of real money cannot be said to return to its original position, since it cannot, by its very definition, ever deviate from it in the first place.

One can easily argue that these difficulties in applying regular economic analysis to money arose only because the analysis was framed in terms of the real money stock. The actual liabilities of the money

issue are really--in a legal sense--nominal units, in much the same way as the physical quantity of commodities is best represented by measurement in real terms. This, of course, coincides with the neoclassical view that what the authorities have a handle on is the nominal money stock but that the real money stock is out of their control and is entirely in the hands of the public. Let us therefore tackle the same exercise afresh, this time using the nominal money stock as our unit of analysis. Again consider a starting position of equilibrium which is disturbed by an increase in the money stock. Neoclassical theory would predict that the nominal money stock will remain permanently at its higher level, and so will the absolute price level. The absolute price level represents the relationship of the set of the relative prices of nominal money now to the set at some reference point in time. This means that the theory will predict that the relative price of nominal money will remain permanently at its higher level.

In other words, unlike the commodity industry, the money industry incorporates no mechanism which would correct a deviation from its equilibrium position. If the issuer of money responded to market stimuli in the fashion of the commodity producer who was discussed above, then we could start at a position representing equilibrium to the producer; any deviation from this position not warranted by change in any of the exogenous variables or parameters would lead to further changes which would in turn evoke corrective action on the part of the producer, back to the old equilibrium.⁹

Instead the nominal money stock cannot be said to be in, or out of equilibrium: it is an exogenous variable. This has implications for Latsis' views on the situational determinism of neoclassical

theory. In neoclassical monetary theory, there certainly is situational determinism on the demand side for money. The demand is for real balances and is treated on a par with the demand for commodities: it is subject to the utility-maximizer's choice criteria as manifested in the utility map. The model is built in such a way that the "logic of the situation" requires the utility-maximizer to go to the intersection of the real output line and the expansion path, in the same manner as the perfectly competitive firm goes to the lowest point of the average cost curve. On the supply side, however, the money stock should be viewed only in nominal terms and here, situational determinism cannot be said to apply. Viewed in nominal terms, the money supply is not determinate. The criteria for its determination are indicated.

How could such an important variable as the money supply, the size of which is nowadays subject to so much debate, be put, in neoclassical theory, on a par with rainfall or the plague? The question of the money supply, as important as it is today, is irrelevant within the terms of neoclassical theory.¹⁰ We saw above that the consequences of monetary changes were of little significance; they are restricted to absolute prices and had nothing to do with the important variables, namely relative prices and output. This theory is the product of an age when the economy was not studied in macroeconomic terms. Money is, after all, a macroeconomic variable, and the situational determinism talked about by Latsis is microeconomic. It applies only to microeconomic agents.

Conclusion

This chapter was a discussion of neo-Walrasian monetary theory as

part of the neoclassical program to which it belongs. The chapter also elaborated on situational determinism which characterizes the monetary behavior of microeconomic agents. This situational determinism does not apply, however, to the supply of money. The neoclassical theory has nothing to say about the determination of the quantity of money, which is, in any event, a variable of minor relevance to the model. It bears stressing that the agents mentioned in the neoclassical program are strictly microeconomic agents.

The reader will recall that an instruction in the neoclassical positive heuristic in Chapter 1 asked the theorist to make the model as realistic as possible. In pursuit of this objective, Clower and Burstein, for example, adapt the Archibald and Lipsey model presented diagrammatically above (see section on the real balance effect, page above) to an economy which has financial assets and liabilities.¹¹ Financial intermediation leaves the neutrality of money intact. Despite adaptations of this kind, the neoclassical program is limited in its applicability to practical, macroeconomic policy issues. The following chapter looks at radical, macroeconomic departures from the program, and attempts to construct a Keynesian program as a counterpart to the neoclassical.

Chapter Notes

- ¹This section puts together some of the ideas contained in D. Patinkin, Money, interest and prices, an integration of monetary and value theory, 2nd edition, New York: Harper and Row, 1965 (part 1); F. H. Hahn, "The rate of interest and the general equilibrium analysis," Economic Journal, 1955, 65, pp. 52-56; G. L. S. Shackle, "Recent theories concerning the nature and rôle of interest," Surveys of economic theory, volume 1, prepared for the American Economic Association and the Royal Economic Society, London: Macmillan, 1965. Other relevant surveys are contained in H. G. Johnson, "Monetary theory and policy" and "Recent developments in monetary theory," both reprinted in R. S. Thorn, Ed., Monetary theory and policy, major contributions to contemporary thought, New York: Praeger, 1976.

- ²P. Mini, Philosophy and economics, Gainesville, Florida: University Presses of Florida, 1974, especially Chapters 6 and 7.

- ³There is another dichotomy within the first six equations. The subset (1) through (3) determines s , i and r , while q , L and W/P are determined separately in equations (4) through (6).

- ⁴If the economy had only one commodity, money may or may not be needed. But what is being discussed here is a model with only one commodity, where a number of commodities is collapsed, for purposes of simplification, into one.

- ⁵The next four paragraphs reproduce the neo-Walrasian equilibrium analysis contained in G. C. Archibald and R. G. Lipsey, "Monetary and value theory: a critique of Lange and Patinkin," Review of Economic Studies, 1958, 26, pp. 1-22. The diagrammatic presentation may encounter the objection that the vertical axis represents a time-independent stock entity, while the horizontal axis measures a time-dependent flow. However, there is nothing wrong in entrusting the rational agent with a choice between a stock and a flow.

- ⁶J. R. Hicks, Value and capital, 2nd edition, Oxford: Oxford University Press, 1946.

- ⁷P. A. Samuelson, "What classical and neo-classical monetary theory really was," Canadian Journal of Economics, 1968, 1, No. 1, pp. 1-15.

- ⁸For the sake of completeness, this note adds a general equilibrium model for a monetized economy (Model 2 below), patterned after P. A. Samuelson, (op. cit.) and two additional models for comparison.
 Model 1 contains six equilibrium conditions for a nonmonetized economy where capital (K) is the numeraire, and solves for 6 unknowns: P_1/P_K , P_2/P_K , W/P_K , r , Q_1 , Q_2 . The P 's stand for nominal money

Chapter note 8, continued

Model 1

$$\frac{P_1}{P_K} = \left(\frac{\overline{MC_1}}{\overline{MC_K}} \right) \quad (1)$$

$$\frac{P_2}{P_K} = \left(\frac{\overline{MC_2}}{\overline{MC_K}} \right) \quad (2)$$

$$\delta f / \delta L = W / P_K \quad (3)$$

$$\delta f / \delta K = r \quad (4)$$

$$\frac{U_{Q1}}{U_{Q1}Q_1 + U_{Q2}Q_2} = \frac{P_1/P_K}{WL/P_K + rK} \quad (5)$$

$$\frac{U_{Q2}}{U_{Q2}Q_2 + U_{Q1}Q_1} = \frac{P_2/P_K}{WL/P_K + rK} \quad (6)$$

Chapter note 8, continued

Model 2

$$\frac{P_1}{P_K} = \left(\frac{MC_1}{MC_K} \right)$$

$$\frac{P_2}{P_K} = \left(\frac{MC_2}{MC_K} \right)$$

$$\delta f / \delta L = W / P_K$$

$$\delta f / \delta K = r$$

$$\frac{U_{Q1}}{U_{Q1}Q_1 + U_{Q2}Q_2 + U_M M} = \frac{P_1 / P_K}{WL / P_K + r(K+M)}$$

$$\frac{U_{Q2}}{U_{Q1}Q_1 + U_{Q2}Q_2 + U_M M} = \frac{P_2 / P_K}{WL / P_K + r(K+M)}$$

$$\frac{U_M}{U_{Q1}Q_1 + U_{Q2}Q_2 + U_M M} = \frac{r}{WL / P_K + r(K+M)}$$

$$M = \overline{M}$$

Model 3

(1) Same as in Model 2.

(2) " "

(3) " "

(4) " "

(5) " "

(6) " "

$$(7) \frac{U_M}{U_{Q1}Q_1 + U_{Q2}Q_2 + U_M M} = \frac{1 / P_K}{WL / P_K + r(K+M)}$$

(8) Same as in Model 2.

prices. W is the nominal wage; r is the interest rate; the Q 's are the two final commodities. The two factors of production, L (labor) and K are in fixed supply; replacement capital maintains the capital stock at a constant level. The production function is of the Ramsey-Solow type; the ratio of the marginal cost of each commodity to the marginal cost of capital is constant. Hence the derivation of marginal equivalences (1) and (2). U represents the utility function; f the production function.

Monetization requires the inclusion of a new set of utility maximizing equivalences: (5), (6), and (7) in Model 2. U is homogeneous of degree zero in M and all the P 's. In this model, equations (1) through (7) contain seven unknowns: P_1/P_K , P_2/P_K , W/P_K , r , Q_1 , Q_2 and M/P_K . The addition of the eighth equation introduces the money supply and allows for the solution of a further unknown: in place of the seven unknowns above, we can now solve for P_K , P_1 , P_2 , W , r , Q_1 , Q_2 , and M . In other words, equation (8) determines all absolute prices.

We shall now have a closer look at factor prices in Model 2. Here W/P_K represents the rate of exchange of labor for final goods (expressed in capital units) in the factor marketplace. Equation (3) states that this rate of exchange will equal, in equilibrium, the rate of exchange of labor for output (expressed in capital units) in the production process. Likewise, the interest rate r represents the amount of output (expressed in capital units) earned by one unit of capital per unit of time, and equals (in equation (4) the rate of exchange of capital per unit of time for output (expressed in capital units). Note, however, that the interest rate serves in this model also as the cost of money, in contrast to our earlier procedure where we had the price level fulfill that function. This dual role of the interest rate rests on the notion of endowing money with the function of a store of value. The interest rate represents, for Samuelson, the return on alternative forms of wealth: in holding money, the wealth holder loses the flow of services he would have earned from these. The absolute price level, in contrast, represents the quantity of the composite good that has to be foregone in order to obtain a unit of money.

Whether the interest rate or the absolute price level is the true price of money is a thorny issue, which will not be discussed here. Should r in equation (7) of Model 2 be replaced by $1/P_K$, we would end up with Model 3. Here, the first seven equations contain seven unknowns: P_1/P_K , P_2/P_K , W/P_K , r , Q_1 , Q_2 , and M/P_K , the same set of unknowns as in the first 7 equations of Model 2. The addition of the eighth equations permits, as before, the determination of absolute prices.

Because of the homogeneity postulate, money is neutral in both Model 2 and Model 3.

Of his model, Samuelson writes that, ". . . correct neoclassical theory does not lead to the narrow anti-Keynesian view of those Chicago economists who allege that velocity of circulation is not a function of interest rates." That much having been granted, one hastens to add that, unlike Keynesian theory, neoclassical theory does not allow the money supply to influence the interest rate, even though the interest rate enters into the demand function for money.

⁹On this matter, see also S. Ahmad, "Is money net wealth?," Oxford Economic Papers, 1970, 22, pp. 357-361.

¹⁰One may argue that our earlier account of the monetary process--the part where we looked at the money supply in real terms, and which we later rejected in favor of an analysis in nominal terms--overstated the power of monetary changes. Archibald and Lipsey (note 5 above) use the concepts of the Walrasian auctioneer and tatonnement to analyze the periods between equilibrium. By their account, upon a change in the money supply--suppose the money supply is doubled--the system goes into disequilibrium, trading is suspended and the auctioneer is called to duty. No trading takes place until the auctioneer calls the equilibrium prices, at which point all prices are double their previous-equilibrium level. Also at this point, the real money stock will be back at its original level. By this sort of approach, monetary changes influence what goes on during tatonnement, but have no effect whatsoever on trading. One cannot conclude that monetary changes have no effect on economic transactions, since the dealings of the auctioneer during the tatonnement constitute economic transactions. However, monetary changes have no effect on the actual, "real" trading. This leads Archibald and Lipsey to conclude that "[for] those well-known propositions of the quantity theory which are propositions in comparative statics, the real-balance effect is irrelevant."

¹¹R. H. Clower and M. L. Burstein, "On the invariance of demand for cash and other assets," Review of Economic Studies, 1960, 28, pp. 32-36.

CHAPTER 3

KEYNESIAN THEORY

The controversy over what Keynes really meant is not relevant to our study. In the light of our discussion on methodology in Chapter 1, what is significant is not so much what Keynes said as what he was understood to have said. Social scientists become important not on the basis of what they "really" say, but rather by virtue of their contribution to the direction of thought in their discipline. The work of one author is likely to form only part of a research program, and the success of a program will depend on the generation and later corroboration of testable hypotheses over long stretches of time rather than on the strength of one particular theory or set of theories. It follows that an understanding of the major currents in Keynesian thinking is more important than the literal significance of the General Theory or any other major Keynesian work. This is not, however, to deny the relevance of Clower, Leijonhufvud, Hines and others who sought the "real" meaning of the General Theory, since these authors not only contributed useful insights into Keynesian theory, but also represent a significant, if not major undercurrent in modern economics.

Describing Keynesian theory, to be juxtaposed against its neoclassical counterpart, could be a difficult task. A student educated along Anglo-American lines will probably recognize the transition from the neoclassical to the Keynesian model by a switch from the microeconomic (including a theory of general equilibrium rich in microeconomic detail)

to the macroeconomic, with solutions involving disequilibrium (particularly in the labor market) becoming a routine matter. Fiscal policy, while not excluded from neoclassical theory, becomes more of a panacea. Monetary policy, which was quite impotent in the neoclassical model, becomes theoretically significant, but very serious doubts emerge as to its efficacy.

Keynesian theory as we have come to know it is certainly no thoroughbred. Mating it with neoclassicism was a major effort of Alvin Hansen, Bent Hansen, Hicks and others, and the success of the breeding may account for our difficulties in spelling out what makes a theory Keynesian. Take for example, Bent Hansen's variant of Klein's version of the Keynesian model.¹ Klein's original is on the left; as rewritten in a supply-and-demand format by Hansen on the right.²

$S/p = f(r, Y/p)$	$C/p = f^*(r, Y/p)$
$I/p = g(r, Y/p)$	$I/p = g(r, Y/p)$
$I = S$	$Y/p = C/p + I/p$
$M/p = L(r, Y/p)$	$Y/p = q^S$
$Y = pq$	$q^S = q^S(N^d)$
$q = q(N)$	$N^d = q^{-1}(w/p)$
$w/p = q^1(N)$	$N^S = h(w/p)$
$N^S = h(w/p)$	$M^d/p = L(r, Y/p)$
	$M^S/p = \bar{M}/p$
	$M^d = M^S$

The addition of a labor-market equilibrium equation ($N = N^S$) would transform either model into a neoclassical model. If instead we add an identity defining the exogenously determined wage, we end up with a Keynesian model. The one on the right would have 11 equations to solve 11 unknowns, among which would be the price level. Indeed, if we re-write all the equations in real terms and omit the price-level variable we are left with the spectacle of a Keynesian model which is overdetermined for lack of a price level variable. Hansen sought to demonstrate

that rewriting a Keynesian model in a supply-and-demand format would transform it into a special, rigid-wage, case of the neoclassical model.

Before we come to grips with the Hansen model on the right, a few introductory comments are in order. As with the model in Chapter 2, Hansen's neoclassical version can be segmented into two parts: the first seven equations plus the labor market equation determine C/p , r , Y/p , I/p , q_s , N_d , N_s and w/p , while the last three determine M_d , M_s and p . In other words, the model complies with the neoclassical dichotomy of real from monetary variables. While the price level is determined exclusively in the monetary sector, the determination of the interest rate lies squarely in the real sector: the Hicksian IS-LM synthesis (yet to be born, in 1937) is unknown here. The same fundamental question raised in Chapter 2 can be asked again: if p were determined purely in the monetary sector, how can it honestly perform its function as the relative price of goods in terms of money? A true relative price will influence the desired ratios of goods to money, but also will itself be influenced by the supply of either.

The inclusion of income as a determinant of consumption and investment must have raised the eyebrows of many a neoclassicist. Optimal allocation is a matter of relative prices and in the neoclassical tradition, income is no constraint since the economic agent can borrow his way across time periods with complete disregard for his current income. So there is no way to derive logically a general equilibrium model with an income constraint if one's starting point is a partial microeconomic model, as it should be when one is building a general equilibrium model. Fallacy of composition? Hardly; it is true that an entire society, in a closed model (that is, one without international commerce) cannot

borrow or lend, but a general equilibrium model builds from microagent behavior upwards, and intertemporal relative prices in such a model will ensure there is no net borrowing or lending at the macroeconomic level. Clower's observation³ on the addition of income constraints in macroeconomics--that they reflect the recognition that when markets do not clear and "false trading" (that is, exchange at non-equilibrium prices) takes place, less-than-equilibrium or constrained incomes are a necessary independent variable in demand functions--led to Leijonhufvud's well-known reinterpretation of Keynes.⁴

But disequilibria in the Walrasian sense are difficult to discuss in the context of Hansen's Keynesian model, where the labor market is in permanent disequilibrium. By Walras' Law, one other equation should also be out of equilibrium. The excess supply in the labor market will be matched by an excess demand in this other equation for a net excess demand of zero in all markets. But which equation? The money market is said to clear, usually at an equilibrium interest rate. This leaves us with the output market. The problem with this market is that no prices are included among the independent variables in the output demand equations. Indeed, the absence of prices from those two equations would seem to place the Hansen model out of range of Walras' Law. So our suggestion above that prices be included in the output demand equation bears rethinking: the model is damned in neoclassical eyes if the output demand equations lack the price variable, but if a price variable is included, it is still damned, since it will certainly fail the test of Walras' Law, unless it allows for the money or output markets to be in permanent equilibrium.

On the incompatibility of his model with Walras' Law, Hansen writes

that

the answer . . . seems to be that in Keynesian theory, workers are assumed not to expect to sell all the labour which they supply. They expect to sell only what is actually demanded. Their expected income is therefore smaller than the value of their supply of labour and equal to what they actually happen to sell. . . .⁵

Why the expectations of workers with regard to market clearance are formed in a manner different from those of other economic agents is never explained.

The foregoing suggests some logical problems in dressing the Keynesian model in neoclassical clothes, but we need a stronger criterion to help us determine the connection, if any, between the two, or whether the two schools are completely irreconcilable.

Say's Law

Such a criterion can perhaps be found in Chapter 1, particularly after page 9. There, the discussion of Lakatos suggested that different programs generate different testable hypotheses, and then the progress or otherwise of a program depends on whether these hypotheses are corroborated. This procedure is especially suitable for the natural sciences. In the social sciences, however, one has to keep in mind that hypotheses often take the form of policy recommendations and that corroboration could then come in the form of a successful policy outcome. Seen in this light, the Keynesian-neoclassical dispute represents a clash between irreconcilable policy recommendations. It may be seen to revolve around the one crucial policy question: how well do markets fulfill their function of allocating resources? The one fundamental premise underlying neoclassical economics is that they are uniquely suited for the job. Keynesian economics on the other hand casts a

serious doubt. Dressing a Keynesian model in neoclassical clothes may be useful to facilitate the conversion of members of the profession whose hearts belong to the Keynesian philosophy but whose upbringing was elsewhere. In line with our discussion in Chapter 1, a switch in research programs can be especially hard on the old guard. The protective belt of a young progressive program may well veer toward the content of the competing degenerating program; this would make the transition easier for all.

This argument can be usefully recast in terms of Say's Law. By Hutt's interpretation,⁶ Say's Law can be expressed as follows: If prices are allowed to find their own level, no resources will be unemployed. Equivalently, any unemployment of resources can be attributed to some activity which incapacitates the workings of the market and prevents the emergence of equilibrium prices. If Say's Law is accepted, the policy implication would follow that unemployment of, say, labor can be attributed to monopolistic or monopsonistic practices which obstruct the emergence of an equilibrium real wage. Unemployment in such circumstances would be due not to a lack of aggregate demand, but to a deficiency of supply. This belief in the smooth workings of the market goes hand in hand with the recommendation against government superimposing itself on the market, and against monopoly- or monopsony-induced distortions. Expanding government expenditures will not pull the economy out of a recession, since such an expansion would constitute an increase in demand, which is the wrong medicine. Breaking up the monopolies or labor unions would break the stranglehold on the free movement of prices, and expand supply, leading the economy back to full employment.

If Say's Law is not accepted, however, the policy maker would be well advised to add directly to the employment of resources, since in the absence of such government involvement markets cannot be entrusted with the task of finding the appropriate prices for full employment.

The presence or absence of money does not alter the argument in the least from the point of view of the proponents of Say's Law. In the eyes of the supporters of the price mechanism, the concept of withheld demand is absurd. Selling one's products and retaining the proceeds in the form of money rather than buying other people's products can in no way cause unemployment. The most that can happen is the emergence of a new set of relative prices. A new equilibrium will emerge consonant with the structure of preferences. Two underlying premises are transparent: holding money is as rational a choice as that involved in any economic decision; furthermore, the market can handle such an eventuality with as much ease as it can handle any switch in preferences. For the opponents of Say's Law, on the other hand, money is the very stuff of disequilibria. If markets cannot be trusted to generate equilibria, the money market is likely to end up as Walrasian receptacle of the excess demand counterpart to the excess supply of labor and other unemployed resources.

Supporters of the market mechanism are likely to prefer using microeconomic models, since it is only within such a framework that the market can display its ability to allocate resources appropriately. The result is the neoclassical microeconomic research program of Chapter 1. Quirk and Saposnik⁷ might well argue that macroeconomics is a special case of the general equilibrium model, where the complexity of the latter is reduced to a model with one consumer, one producer

plus the government. The fact remains, however, that the intricate web of relative prices and the attendant allocative process is all but lost in Keynesian macroeconomics. Hansen's model above is less than neoclassical in at least that one major aspect.

Keynesian macroeconomics is by its very nature unsympathetic to the workings of the market, and this is reflected in its methodology. Method in Keynes seems to have followed Keynes' reading of his environment; witness this quotation from The Economic Consequences of Sterling Parity:⁸ ". . . we run a risk of reaching the wrong conclusion . . . if we continue to apply the principles of an Economics which was worked out on the hypothesis of laissez-faire and free competition to a society which is rapidly abandoning these hypotheses."

In the language of Simon (Chapter 1 above) the decomposition in Keynesian models stops at a macroeconomic level. For the most part, they ignore microeconomic variables, and as a result, the method of specification ignores the web of microeconomic relationships.

Portfolio Balance

The portfolio-balance method is nowadays very popular among macroeconomists, and its appeal is common to both Keynesians and non-Keynesians. Because of the latter, we shall return to it in the next chapter, but at this stage it is pertinent to mention that Keynes is regarded by Patinkin⁹ as the founder of the concept of the transmission mechanism in the modern sense of the word. Portfolio adjustment is a domino-like effect in the relative price structure of assets and appears to be a sophisticated exercise in price adjustment, rich in disaggregated detail.

Patinkin attributes to Keynes the modern notion of money as a stock. Prior to Keynes, money was perceived as a flow; the demand for money was related to income, itself a flow. The detachment of money from income, and the consequent movement away from the medium-of-exchange function towards the store-of-value function began with the Cambridge equation of exchange and culminated in Keynes, where money started to be treated as a stock. The portfolio owner holds money as well as other assets in a ratio consistent with the price structure of assets, and a change in the volume of money would require a new set of relative prices among assets. As elaborated by Tobin,¹⁰ the particular substitutability amongst the various financial assets and then between these and real, physical assets will determine the transmission of the initial monetary change through a path leading to the ultimate impact on the real economy.

It should be noted in passing, however, that the Keynesian mechanism has none of the complexities of, say, Friedman's model, which will be dealt with in Chapter 4. The Keynesian model can be said to contain three assets: money, financial capital and real capital. The latter two are substitutes. This contrasts with Friedman's mechanism, where the variety of channels and the multitude of variables involved suggests a higher regard for microeconomic detail and the allocative role of relative prices.

Deference to microeconomics and the subjective, individualistic method is evident also in a much earlier work: The Theory of Money and Credit by Ludwig von Mises.¹¹ It must be granted that even though the original German text came out well before the General Theory, portfolio adjustment per se was not alien to von Mises, Patinkin's claims on

behalf of Keynes notwithstanding. Von Mises writes:

He who has more money on hand than he thinks he needs, will buy, in order to dispose of the superfluous stock of money that lies useless on his hands. If he is an entrepreneur, he will possibly enlarge his business. If this use of money is not open to him, he may purchase interest-bearing securities; or possibly he may decide to purchase consumption goods. But in any case, he expresses by a suitable behavior in the market the fact that he regards his reserve of purchasing power as too large.¹²

One must do justice to Patinkin's position and admit that von Mises never talks of a relative asset price structure corresponding to the set of asset stocks. Yet if one reads on, The Theory of Money and Credit, as well as other Austrian-school literature, provide more than a clue as to why such a development had to await the General Theory. As the next section shows, an Austrian-type method would deal with money in a disaggregated fashion, while the macroeconomic portfolio approach (Keynesian or otherwise) deals with the macroeconomic consequences of a monetary change, primarily on the interest rate and on asset prices.

A Microeconomic, Individualistic Treatment of Saving, Investment and Money

Saving can be viewed as the expression of a preference of future goods over present goods. Likewise, investment can be regarded as the manifestation of a preference for present over future goods. In other words, investors demand capital goods at the present time, and will be in a position to supply in the future goods produced with the help of these capital goods. That the demand for present goods is made against the offer of claims on future goods rather than against the future goods themselves does not in itself influence our eventual conclusions. At this stage, therefore, we can ignore the web of financial

intermediation which stands atop real transactions. Instead of a market where saving and investment are equated at an equilibrium interest rate, there is the supply and demand for commodities--as usual, except that commodities are delivered in different time periods, and the prices determined in the various markets are the prices of the commodities each defined as of a particular point in time. The interest rate can now be defined as the ratio of the price of a commodity at a future point in time to the price of the same commodity at the present time. There is no reason why the ratio for one good should be the same as for any other good, except that as the economy moves towards equilibrium differences in such ratios would lead to a reallocation of resources until uniform ratios are established throughout the economy by the time the next general equilibrium is achieved.

An increase in savings can now be interpreted as an increased preference of future over present goods. This changes the rate of exchange of future for present goods. The quantity of present goods exchanging for the same quantity of future goods rises, implying a decline in the rate of interest. For example, whereas before the switch in preferences towards saving, 100 units of a present good exchanged for 106 units of that same good in the future, implying an interest rate of 6%, now 101 units of the present good will exchange for 106 units in the future, equivalent to an interest rate of under 6%. The price of present goods in terms of future goods will fall; the price of future goods rises as will the price of claims on future goods, if we incorporate financial intermediation into our analysis. In other words, the price of an asset and the yield on such an asset move in opposite directions.

Money can be integrated into such an approach without undue

difficulty. Newly-issued money constitutes a claim on goods and services. Here it can be perceived as a flow of claims filtering down to microeconomic agents, and prompting a redistribution within the economy. Whether the interest will rise or fall depends on whether the recipients prefer future goods to present goods or vice versa. It may so happen that if the government, for example, transfers newly-created money to poor recipients of public welfare funds, the consequent redistribution would probably raise the price of present goods in terms of future goods. Conversely the usual prediction of a drop in the interest rate upon an increase in the quantity of money will obtain if a redistribution occurs leading to an increased preference for future goods.

There remains, however, a fundamental difference between an increase in saving which results from a change in preferences amongst economic agents and one which ensues from a redistribution of wealth following an increase in the quantity of money which favors those in whose preference structure future goods feature more prominently. A change in preference leads to a permanent change in the interest rate, reflecting an alteration in the equilibrium rate of exchange of future for present goods. When a redistribution occurs as a result of monetary expansion, on the other hand, the subsequent change in interest rate can be viewed as a superimposition on the economy which is more likely to be reversed as the proceeds of the initial monetary expansion are respent over and over again. A theoretical model with the appropriate institutional detail can be set up to guarantee the long-term neutrality of money.¹³

The distinguishing characteristic of the approach discussed above is perhaps that the interest rate is determined in periods of

disequilibrium as well as in equilibrium, along with all other relative prices, through an intricate web of microeconomic activity. With the appropriate institutional backdrop, more elaborate models can be set up. In Hayek, for example, the effects of a monetary expansion on the methods of production are looked at in detail. Production is broken up into various stages which link present goods at one end to future goods at the other. A switch in preferences towards future goods will not merely raise the price of future goods and make the production of such goods more attractive, but will also make production at any stage of production more attractive relative to earlier stages, thereby prompting a lengthening of the production process. The short-run effects of a monetary expansion can also be viewed through the use of a similar theoretical apparatus.

The Keynesian portfolio approach, in contrast, is essentially a macroeconomic approach, and by virtue of its macro nature, is able to handle money as an aggregate quantity. Instead of viewing an increase in the quantity of money as an increase in the claims (on goods and services) made available to so many microeconomic agents, whereupon the disposal of such claims leads to the eventual repercussions on the real economy, now we can talk directly of a market for money at a macroscale determining a macroeconomic interest rate which in the Keynesian age came to be viewed as the price of money. There is nothing logically incorrect with the asset-price and interest rate movements which the Tobin-Keynesian theory predicts will occur across the macro-portfolio of assets following a monetary expansion. Likewise it is logically acceptable to posit perfect substitutability between financial assets (other than money) and real assets. After all, assuming away

differences in risk, assets enter preference structures only via their rates of return. Yet this new method, which instructs the theorist to analyze economic activity in macroscopic terms has the advantage of viewing money, a macro quantity, in suitable theoretical dimensions, but the disadvantage of possibly missing relevant microeconomic detail.

A Keynesian Research Program

In a Lakatosian reconstruction of Keynesian theories, the positive heuristic veers the theorist away from the microeconomic, in a manner which distinguishes the resulting theories both from the neoclassical types we saw in Chapter 2 as well as the Austrians presented in this chapter. The Keynesian theorist is also required to address himself or herself to policy questions. This mission is not inconsistent with the Austrian school, but is removed from the Walrasian theories of Chapter 2. The Keynesian positive heuristic would read:

- (a) Construct aggregative economic models.
- (b) Minimize the non-economic content.
- (c) Analyze macroeconomic stabilization.
- (d) Adapt the models to make them as acceptable as possible to neoclassicists.

The first instruction sets the macroeconomic tone. The second instructs the theorist to avoid non-economic content. In particular, Keynesian models generally avoid sociological and political detail. The result is a situational determinism, which is different from that of microeconomic models and which will be the subject of the conclusion of this chapter. The third instruction permits the theorist to study systems which to a neoclassicist were unstable or in short-term disequilibria. The fourth heuristic is an exercise in public relations and an appeasement of the old guard.

The hard core of the Keynesian program consists of one proposition:

- (i) Economic activity is the outcome of the interaction of private markets and macroeconomic policy.

History of the Keynesian Program

Had we regarded Keynesian theory as belonging in the protective belt of the neoclassical program, then the 40's, 50's and part of the 60's would have had to be viewed as a time of re-invigoration and progression in neoclassicism, thanks to Keynesian addenda, after the debacle and regression of the Great Depression. If, on the other hand, we view Keynesianism, as we have in this chapter, as a separate research program, the history of its component theories is a good example of a Lakatosian degenerating program. The verdict on the matter depends, of course, on one's particular views of the history of Keynesianism. Here, however, is one attempt at a Lakatosian reconstruction of the development of Keynesian thought.

1. The starting point must be the 45-degree model of Alvin Hansen and Paul Samuelson, which was strictly macroeconomic and which had little to say about the absolute price level at all output positions short of full employment. The model came hand in hand with the recommendation for an active fiscal policy.

2. The Hicksian IS-LM model¹⁴ was primarily a theoretical refinement, although it has since undergone independent testing. In the very early formulations of Keynesianism, the interest rate determined the level of investment, which in turn determined (via the multiplier) the level of income; yet the latter was itself a determinant, in the liquidity function, of the interest rate. The IS-LM technique integrated the system, and an ignominious circularity in logic was transformed into a

model with as many equations as it has unknowns.

3. In Keynesian theory, emphasis is switched away from price adjustment (the sort of adjustment which is the essence of a well-functioning market system) to quantity adjustment. By the very nature of this departure, which plays down the role of price fluctuations as an equilibrating mechanism in the market economy, Keynesian theory should have lost its credentials as a credible explanation of the inflationary process, which consists after all of a sequence of price movements. Since the early Keynesian models downplayed the role of price changes, let alone came up with adequate proposals for controlling inflation, the protective belt eventually had to be adjusted, to incorporate formally the rate of inflation into the program. The program came up with additional testable hypotheses and was in a position to progress if these hypotheses were corroborated.

Despite the potential inconsistency, Keynesian theory by its very formulation lends itself quite readily to an investigation of the inflationary process. Firstly, it operates in easily understood terms of aggregate supply and aggregate demand, and by bringing price adjustment back into the picture, Keynesian theory can easily attribute inflationary pressures to excess aggregate demand. Secondly, Keynesians often presume an exogenous money wage rate and what market forces do not determine--the wage rate--easily becomes a function of a variety of "non-economic" candidates. Hence the "cost-push" theories of inflation. As a result we find two kinds of Keynesian explanation of the inflationary process. With the quantity theorists, Keynesians shared the demand-pull explanation, albeit of a non-monetary type. In conflict with the quantity theorists, Keynesians had a cost-push explanation

totally at odds with the idea that only market pressures pull prices up. The two explanations lead to diverging policy recommendations. The demand-pull variant called for a reduction in aggregate demand as a corrective; the cost-push variant called for a repression of the relevant causal factors. The difference between the two however is not as wide as it seems. Both remedies are of a non-monetary nature. Besides, the two explanations are empirically indistinguishable: any opponent of the cost-push explanation is bound to be open-minded enough about the matter of lags to attribute seemingly cost-push price movements at any point in time to demand pressures any length of time before.

The cost-push version was undoubtedly the worse of the anathema from the quantity theorists point of view. Any delving into spontaneous or exogenous (rather than market-pressure induced) change in prices or wages required explanations outside the boundaries of conventional economics. Referring to a term coined by Thorp and Quandt, Bronfenbrenner and Holtzman write that

the term "new inflation" embodies the opinion that the strength of economic pressure groups . . . , together with increased public concern with unemployment, had increased the likelihood . . . of "disequilibrium" price and wage increases being validated by expansive monetary and fiscal policies, resulting from organized pressure on monetary and fiscal authorities.¹⁵

This sounds like the starting point of a new sociological or political approach to inflation, but what follows in the same article is an apologetic review (reminiscent of heuristic d on page 66) of what circumstances would cause a cost-push deviation from the perfectly competitive norm. The discussion deals with such issues as price inelasticity in the product market, and low factor-market elasticity of substitution, which would permit cost-push pressures to succeed. The major motivation

behind trade union pressure on wages is said to be not the cost-of-living increases or the profits earned by the employer, but rather "neighboring strategic wage rates," and then only in the absence of perfect labor mobility.

A major development in the Keynesian treatment of inflation was the appearance in the protective belt of the Phillips curve,¹⁶ which could be regarded as a Keynesian theory of inflation in its own right, with its own attendant hypotheses. The program again came up with new testable hypotheses. An alternative, somewhat cynical view would suggest that the inability of Keynesian-oriented politicians to solve the problem of unemployment led theorists to propose a model indicating the necessity for some unemployment if inflation were to be kept in check. By this view, refutation came in the form of policy-failure. Inability to solve the problem of unemployment led to a minor switch in policy. Loyalty to an overall Keynesian philosophy remained, but the full-employment objective now meant less than strictly full employment. In pseudo-Lakatosian terms, a minor change in policy accompanied a change in the protective belt, in the same way as a major, radical change in policy (such as occurred in the 1930's and 1940's) perhaps accompanies a change in the hard-core itself, and therefore a switch in programs.

4. The next watershed in the Keynesian saga came with the episode of the shifting Phillips curves.¹⁷ When the simultaneous deterioration in inflation and unemployment conflicted with the Phillips hypothesis, the protective belt was repeatedly modified to appoint expectations, trade union power and so on as shift variables in the Phillips relationship. The Phillips curve was able to explain everything after the event, but its predictive abilities were now inadequate. Ad hoc

modifications helped to salvage temporarily the Phillips hypothesis, but, as a decompositional and specificational device which isolated the interaction of wage and price movements, it was refuted; the Keynesian program was again degenerating. Insofar as corroboration or refutation are at least partly a function of the events of the time, the environment of theorists and econometricians has a lot to do with the health of a program.

5. Degeneration in the program was evident in Tobin's presidential address to the American Economic Association in 1971.¹⁸ The message was "Ignore inflation and concentrate on the full-employment objective." Here was an ill-fated attempt to revert to the specificational procedures of the 1940's, when it was politically safe to ignore inflation. The Keynesian program seems to be approaching another attempt at resuscitation: Klein's address to the A.E.A. in 1978 called for more consideration to be given to aggregate supply in macroeconomic stabilization policy.¹⁹

Conclusion

The propositions of the neoclassical research program formulated by Latsis (and reproduced in Chapter 1 above, page 15) are designed for the generation of microeconomic models. In particular, the decision-makers of proposition (i) of the hard-core are obviously very well-informed microeconomic agents. As such, this proposition is not suitable for the generation of Keynesian models, where macroeconomic agents--or policy makers--are knowledgeable about the economic situation, but where microeconomic agents, to the extent they are included at all in such models, may not be. (Hence, for example, the neoclassical interpretation of

microeconomic behavior in the conventional Phillips curve scenario suggests that trade unionists are poor predictors of the future path of the price level.) Also, proposition (iv), asserting the self-regulating nature of the economy, is out of place in a Keynesian program, which highlights the need for macroeconomic intervention to stabilize the economy.

As I pointed out in the section entitled "Say's Law" in this chapter, Keynesian theories compete, in a Lakatosian sense, with neoclassical microeconomic theories. The positive heuristic of one is different from that of the other: the Keynesian theorist is directed to a theoretical apparatus different from that of the neoclassical microeconomic economist. In addition, the nature of the recommendations differ. Keynesian theories, unlike their neoclassical microeconomic counterparts, generate short-term stabilization policy recommendations for macroeconomic policy-makers.

The next task was to suggest a hard-core and a positive heuristic for a Keynesian research program. The positive heuristic of the Keynesian program instructs the economist to concentrate on aggregative relationships. The philosophy as well as the method of the program reflect the dethronement of the microeconomic approach and the accompanying belief in the workings of the market system. Keynesian macroeconomics is better suited for the study of stabilization policy and became necessary with the growing stature of government in modern economies.

Chapter 1 discussed the situational determinism manifested in neoclassical microeconomic theory. The typical microeconomic model is set up in such a way that the economic agents will automatically resort to the point of equilibrium. This type of arrangement was called

situational determinism, and is facilitated by the use of the substantive, rather than the procedural form of rationality, and by the abstraction from sociological and other non-economic detail. Notice that in macroeconomics too, the abstraction from non-economic--particularly political and sociological--detail allows the economist to make policy recommendations, and if the policy-maker does not follow these recommendations, he is often said to have made a mistake. The rationality presumed of the policy-maker is, again, of the substantive type and in these models, the policy-maker typically finds himself in a problem where the logic of situation dictates a unique solution. Occasionally the problem is more complex, as when, for example, the policy-maker faces the dilemma of the simple Phillips curve. Economics, however, has little to say in this situation about whether the policy-maker should opt for less inflation or less unemployment. But once that choice is made, probably on political grounds, the path to that end is clearly laid down by economic theory.

In the more straightforward situation, the macroeconomist typically recommends, say, a cut in government expenditures to the newly elected politician, whose pre-election promises might inhibit his ability to accept the economists' recommendation. The situational logic of this theorist's recommendation is of course valid within the boundaries of economics, but may be unacceptable to the politician who has to operate outside those boundaries.

Chapter Notes

- 1B. Hansen, Lectures in economic theory I. General equilibrium theory, 3rd edition, Lund: Studentlitteratur, 1968, Chapter 12: "Keynes and the neoclassics"; L. R. Klein, The Keynesian revolution, New York: Macmillan, 1947.
- 2Glossary: S = saving; I = investment; Y = national money income; r = rate of interest; p = price level; w = money wage rate; M = quantity of money; q = physical volume of national product; N = employment; N^S = supply of labor.
- 3R. W. Clower, "The Keynesian counter-revolution: a theoretical appraisal," in F. H. Hahn and F. Brechling, Eds., The theory of interest rates, London: Macmillan, 1965.
- 4A. Leijonhufvud, On Keynesian economics and the economics of Keynes: a study in monetary theory, New York: Oxford University Press, 1968.
- 5B. Hansen, op. cit., p. 90.
- 6W. H. Hutt, A rehabilitation of Say's Law, Athens: Ohio University Press, 1974. See also S. Pejovich and D. Klingaman, Eds., Individual freedom: selected works of William H. Hutt, Westport, Conn.: Greenwood Press, 1975, Chapter 3; and W. H. Hutt, Keynesianism, retrospect and prospect, Chicago: H. Regnery Co., 1963.
- 7J. Quirk and R. Saposnik, Introduction to general equilibrium theory and welfare economics, New York: McGraw-Hill, 1968.
- 8J. M. Keynes, The economic consequences of sterling parity, New York: Harcourt, Brace and Co., 1925, pp. 23-24.
- 9D. Patinkin, "Keynesian monetary theory and the Cambridge School" in H. G. Johnson and A. R. Nobay, Eds., Issues in monetary economics, New York: Oxford University Press, 1974.
- 10J. Tobin, "Money, capital and other stores of value," American Economic Review (Proceedings), 1961, 51, pp. 26-37.
- 11L. von Mises, The theory of money and credit, Trans. by M. E. Batson, New Haven: Yale University Press, 1953.
- 12L. von Mises, ibid., p. 134.
- 13See G. P. O'Driscoll, Economics as a coordination problem: the contribution of Friedrich A. Hayek, Kansas City: Sheed Andrews and McMeel, 1977, especially pp. 80, 81. O'Driscoll writes that, "in the case of a shift in the savings function, the resulting changes involve a

transition to a new kind of equilibrium of the kind assumed in barter theory." The long-run results of monetary expansion are different. As F. A. Hayek writes (Prices and production, London: Routledge and Paul, 1935, p. 57), in this case, "[the] sacrifice is not voluntary, and is not made by those who will reap the benefit from the new investments. It is made by consumers who, because of increased competition from the entrepreneurs who have received the additional money, are forced to forego part of what they want to consume. . . . There can be no doubt that, if their money receipts should rise again, they would immediately attempt to expand consumption to the usual proportion."

In an analysis which disagrees with Hayek, von Mises asserts that money is non-neutral. For money to be neutral, he argues, the marginal utility of money has to be inversely proportional to the size of the money balances. L. S. Moss (in "The monetary economics of Ludwig von Mises," in L. S. Moss, Ed., The economics of Ludwig von Mises, towards a critical reappraisal, Kansas City: Sheed and Ward, 1976) disagrees, pointing to Patinkin's findings (in Money, interest and prices: an integration of monetary and value theory, 2nd edition, New York: Harper and Row, 1965, pp. 50-59) that money is neutral even though the demand curve for money is not a rectangular hyperbola. The real balance effect shifts the demand curve and the resulting intersections of supply and demand curves form the market equilibrium curve which is indeed a rectangular hyperbola. For the record, one must add that Patinkin's assertion is disputed by G. C. Archibald and R. G. Lipsey (in "Monetary and value theory: a critique of Lange and Patinkin," Review of Economic Studies, 1968, 26, pp. 1-22), and in turn Samuelson disagrees with Archibald and Lipsey (P. A. Samuelson, "What classical and neoclassical monetary theory really was," Canadian Journal of Economics, 1968, 1, No. 1, pp. 1-15).

- 14J. R. Hicks, "Mr. Keynes and the 'classics': a suggested interpretation," Econometrica, 1957, 5, pp. 147-159.
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CHAPTER 4

THE CONSERVATIVE WING

A major development in economics since the War, aside from the Keynesian revolution, has been the later rise of monetarism. The profession has become a battleground between Keynesians and monetarists, with the latter appearing to gain the edge as inflation becomes a more pressing political issue, and in also line with the changing political mood. This chapter deals with monetarism and with a later development--the emergence of the rational expectations hypothesis, which is, at once, distinct from monetarism but in agreement with monetarism on major policy recommendations. This chapter will evaluate and compare these two components of the conservative wing of economics--to each of which is devoted a separate section. The third and final section attempts to fit each of the two schools into their respective research programs.

Monetarism

Below is a list of the distinguishing characteristics of monetarism, at a theoretical, rather than policy implementation level. The purpose of this section is to link these characteristics with neo-classical microeconomics and with Keynesianism.

1. The Inherent Stability of the Economic System. With its foundations in general equilibrium analysis, this characteristic is manifested in the monetarist assertion of a natural rate of unemployment,

the stability of the demand function for money, the inclusion of permanent (rather than measured or current) income in all demand functions, the exogeneity and the long-run neutrality of monetary aggregates (an assertion founded in the zero-homogeneity in the prices and income of the money-demand function), and the disavowal of policy activism of any sort.¹

Their condemnation of all forms of policy interference with market processes leads monetarists to denounce the size of government spending in modern-day budgets. The expansion in the size of the government leads, in the eyes of many monetarists, to the crowding out of the private sector, through inflationary or non-inflationary means. Recourse to the former route explains the persistence of inflationary problems, which arise primarily from the nexus of the growing size of government and the financing thereof through monetary expansion. At the fore of the theoretical bridge-building between monetary and fiscal expansion is the Brunner and Meltzer brand of fiscal monetarism.²

In the line with their belief in the intrinsic stability of market systems, monetarists downplay the contribution of external shocks to the rate of inflation. Accordingly, they exclude increases in the prices of raw materials, agricultural price fluctuations and the like, as sources of inflationary pressures. Such factors, they say, induce changes only in relative prices and resource allocation, but have no effect on the rate of inflation, unless they are accommodated by monetary expansion.

2. The Separation of the Allocative from the Aggregative. In

simpler terms, the microeconomics of resource allocation and relative prices are compatible with, but distinct from macroeconomics, which deals with aggregate levels of nominal variables, including the absolute price level. The separation is reminiscent of, and indeed it is based upon the Quantity Theory and the superimposition of the equation of exchange upon a general equilibrium foundation. The dichotomy shows up in the reliance of most econometric monetarist literature on tests of reduced-form equation systems which abstract from the sectoral and allocative detail typical of Keynesian structural-equation systems.³ (This particular point is discussed again at a later page.)

An inevitable consequence is the lack of emphasis by monetarists on sectoral contributions to the inflationary process and on the distributional effects (among sectors and income groups) of monetary policy and its inflationary effects. From the monetarist viewpoint, a primary distributional consequence of inflation is the shift of resources from money holders to the issuers of money. This effect is studied in the context of a function relating the demand for money, in real terms, to its determinants. Agents in monetarist economics are considerably more rational, and have access to a far wider range of information than their Keynesian counterparts. The demand for real money balances depends partly upon the expected rate of inflation; this is because inflation reduces the real value of money balances and is therefore an important cost incurred by those who hold money. Expectations of future inflation are in turn a function of the actual, current rate of inflation. As a result, a rise in the rate of inflation, to the extent that it becomes internalized in expectations of future inflation, increases the cost of holding money, and therefore reduces the level of desired money

holdings. Such desired holdings, however, are measured in real terms, and as a result monetarists point to a separate and conflicting effect: money holders will maintain a desired, real stock by accumulating nominal balances at the same rate as the actual rate of inflation. The second of these effects constitutes a tax on money holders, the so-called inflation tax, the proceeds of which accrue to the issuers of money.⁴

3. Its Macroeconomic Nature, manifested by its emphasis on aggregate variables and its use of the macroeconomic portfolio-balance method. This characteristic is not usually mentioned by monetarists in describing themselves, but it represents a major departure from the neo-Walrasian monetary theory discussed in Chapter 2, and so it is pertinent to our discussion. The first two characteristics link monetarism to its foundations; this characteristic asserts its primary purpose, namely the study of the macroeconomy and the short-run effects of policy. These three qualities sum up some major aspects of monetarism: like Keynesian theory, it deals with short-run, macroeconomic policy evaluation, but unlike Keynesian theory, it has deep microeconomic roots. Monetarism is founded upon the equation of exchange, carried over from the old Quantity Theory. Like the quantity theorists, monetarists believe in the exogeneity of the money supply.

There are however subtle differences between the old and the new, reflecting pre-Keynesian but especially Keynesian influences on the interpretation of equation of exchange. The old Quantity Theory stressed the long-run neutrality of money and by the same token restricted the scope of its analysis to the influence of monetary changes on the long-run determination of the price level: in the long-run monetary changes

leave relative prices and all other "real" variables unchanged. The modern variant, enunciated by the monetarists, represents a significant compromise with the Keynesian concern with the short-run and concentrates on questions dealing with short-run disequilibria. Monetarists acknowledge the short-run non-neutrality of money, and search for the conditions which would ensure the short-run neutrality of policy. Some writers deny that short-run, disequilibrium periods were previously ignored in economics; Blaug,⁵ for example, points to the writings of Fisher and Marshall as places where short-run issues were indeed studied. Yet the strong emphasis on disequilibria typical of modern macroeconomics emerged with Wicksell, came to a head with Keynesianism, and is also contained in monetarism, which is in large part the analysis of the effects of monetary changes in the short run on both prices and employment, in other words on both nominal as well as real variables.

With these first three characteristics in mind, a comment is now in order about the unity of monetarist thought. There is disagreement in the literature about this point, namely the question of how far one can deviate from mainstream monetarism and still retain one's monetarist credentials. Take, for example, the use of small, reduced-form models by monetarists. Milton Friedman pointed out, in private correspondence with Mayer,⁶ that the disputes between large and small models are "almost entirely independent of the monetarist versus Keynesian points of view." Mayer disagrees, and lists various reasons why small models are intrinsic to the monetarist model. One important reason is, of course, the monetarist emphasis on the aggregative and their de-emphasis of the allocative and, therefore, the sectoral. Also their

belief in the inherent stability of the private sector permits monetarists to ignore the role of the private sector, and parts thereof, as sources of instability.

Elsewhere, however, Mayer misses various other aspects of monetarist unity. For example, while recognizing that the monetarist's aggregative, non-sectoral approach to the determination of the absolute price level is consistent with the monetarist principles, he remarks nevertheless that the monetarist "rejection of all cost-push phenomena may well be unwarranted even within the monetarist framework." Suppose, he writes, that one industry increases its total receipts by pushing its prices upward, and then suppose that with a constant money supply (and therefore constant total expenditures), other industries react to shrinking demand by reducing production rather than by cutting prices. The result would be an inflation without monetary expansion--a result, says Mayer, which is entirely compatible with monetarist premises. Maybe, but monetarists are unlikely to argue along Mayer's lines. They believe in a stable private sector, a stability based on a general equilibrium model which presumes adequate forces of competition across the economy. Such a presumption would preclude the first industry from getting away with its price heist and would discourage the quantity (as against price) adjustment of other industries. There is, I believe, an undeniable unity among these first three characteristics of monetarism. Perhaps they can be summed up into a definition of monetarism as an aggregative or macroeconomic interpretation of neoclassical theory, concerned with short-term and policy-related issues. It is a policy-maker's cookbook, entirely compatible with neoclassical economics.

The next pages deal with the fourth characteristic of monetarism,

and the one from which it derives its name.

4. Despite their acknowledgement of the confluence of monetary or fiscal policy, monetarists believe that the former is more potent, in the sense that its effects are less ambiguous and more reliable. What is being discussed is not the long-term influence of monetary changes--what Stein⁷ calls the Long-Run Quantity Theory--but rather the short-term potency of monetary changes. Stein distinguishes between Friedman's argument to the effect that monetary changes are the primary cause of changes in nominal income over the long-stretch and the St. Louis thesis which stresses the short-term potency of money. It is the latter variant which is being discussed here. Related to this point is the monetarist assertion of the exogeneity of monetary aggregates.

The first three characteristics of monetarism can be related, in a straightforward fashion to antecedents in economic thought. This last one and the attendant issues of the transmission process, however, are more problematic, and are dealt with at some length in the following few sections.

The Potency of Monetary Policy

As suggested in point 2 of the previous section, a significant part of the monetarist literature lacks detailed theoretical specification and seeks validation for its generalizations directly from empirical support. For example, Friedman and Meiselman provide the generalities of a transmission mechanism but they do not present a formal model. The St. Louis model consists of testable reduced-form equations which are not derived from a larger set of structural equations. The absence of a detailed formal specification is not only consistent with, but is

indeed a consequence of an additional aspect of the monetarist philosophy (not mentioned above) which is reminiscent of the Austrian school discussed earlier: policy decisions influence the private economy through a wide variety of very intricate channels, a variety which eludes simple formalization. The process of specification--in other words, the task of selecting the more important relationships between the variables included in a model--is avoided in what is perhaps a monetarist reaction to Keynesian method. As suggested earlier, monetarists generally rely, in their empirical method, on the reduced-form procedure, which merely tests the dependence of aggregate quantities on various policy variables, but without starting from, and certainly without testing, a structural array of equations. Friedman's instrumentalism⁸ is a supporting methodology: what matters is not the realism of the postulates of the theory, nor the degree of structural detail in its formal models, but merely how accurately the theory predicts.

All this notwithstanding, two major monetarists--Brunner and Meltzer--have published a long string of articles, containing formal and intricate macroeconomic models. Their analysis is in terms of distinct periods. In one Brunner⁹ article, the shortest period sees portfolio reallocation and asset price adjustment after the issue of additional financial instruments, such as money or securities. The analysis is devoted exclusively to the response within the asset markets. For this first period, Brunner suggests that the wealth effects of increased asset supplies are small, and may therefore be ignored.¹⁰ Next, the consequences of the first period are fed into the government budget process and the process of financial stock-flow adjustment in an "intermediate period" is then studied. The final period introduces the limitations

imposed by the real aspects of the economy; the contributions of fiscal arrangements to long-run equilibrium are also analyzed.

Tobin¹¹ believes that portfolio adjustment theory--in particular its assertion "that non-monetary events . . . will also affect the attractiveness of accumulating capital assets"--ought to convince monetarists that they are wrong in attributing so much strength to monetary policy. But if one looks closely at the first of these three Brunner periods, one finds an important part of the monetarist explanation for the relative potency of monetary policy.

Wealth owners allocate their holdings over a range of assets, but in equilibrium the distribution must be consistent with the set of asset yields or prices. Policy changes may upset the portfolio balance and cause changes in asset prices, which would in turn prompt further changes in the real economy. A decrease in the credit-market interest rate reduces borrowing costs and has expansionary consequences, while a reduction in the price of claims on existing real capital is contractionary to the extent that such a reduction prompts substitution away from new real capital.

While the portfolio-balance method is certainly not unique to monetarists, the nature of the portfolio relationships assumed by Brunner and Meltzer is. They include three separate asset markets: those for money, credit and physical capital. The credit market encompasses the demand for and supply of credit, or equivalently, the supply of and demand for credit instruments, such as government securities. The market for physical capital consists of the demand for, and supply of claims on already-existing real capital, namely equity capital. The inclusion of the credit market makes for added institutional realism. Open-market purchases of government securities by the central bank, for example, lead

not only to an excess supply on the money market, but simultaneously to an excess demand on the credit market.

Take an increase in the supply credit instruments, such as might follow from an increased fiscal deficit. Assume that the credit market interest rate, RS , is determined primarily on the credit market, and the yield on real capital, RK , is determined mainly in the money market. Thanks to Walras' law (Chapter 2), we can ignore the real capital market. (As mentioned above, wealth effects can be ignored for the purposes of this short run.) The increased supply of securities on the credit market will raise RS , prompting a reduction of demand on the money market, and therefore a lower RK . The increase in RS has a contractionary effect; the decline in RK has an expansionary effect. Assuming away later feedback between the two markets one concludes that these conflicting results cast a doubt on the effectiveness of fiscal policy, since the net portfolio effect of a debt-financed increase in government expenditures could be contractionary.

The consequences of monetary policy are as intricate. Take an expansion in money supply, and to avoid the complications of open market operations, assume that it is delivered by a helicopter drop. Again assume that RK is determined mainly on the money market, RS on the credit market. The increase in the money supply will reduce RK . The demand for securities will decline, bringing down RS . The reduction in both RK and RS has an expansionary effect. Note however that these results depend crucially on the nature of portfolio relations assumed. Take the opposite, extreme case, where RS is determined on the money market, while RK is determined on the credit market. The increase in money supply will reduce RS , and the consequent rise in demand for

securities will pull RK upwards. The latter effect is, of course, contractionary. Monetary policy is no longer unambivalently expansionary.

Brunner and Meltzer presume strong substitutability between money and real capital, and weak substitutability between money and other financial assets--a presumption which contributes to their assertion of the effectiveness of monetary policy. Alternative assumptions--particularly the assumption of proximity of money and the credit markets--lead to very different results.

There is, of course, more to the monetarist argument than these substitutability assumptions. B. M. Friedman presents this straightforward overview, in the context of a discussion of the strength of fiscal policy.

In a general model including money, bonds, and capital, there is no justification for presuming a priori whether the portfolio effect associated with bond-financed government deficits offsets or reinforces the familiar income effect of fiscal policy.

Whether this portfolio effect is positive or negative depends on a crucial but simple relative substitutability condition: portfolio crowding out (or crowding in) results when the ratio of the substitution coefficient between bonds and money to the substitution coefficient between bonds and capital is smaller (greater) than the ratio of the respective wealth coefficients of the demands for money and capital. If the two ratios are precisely equal, there is no portfolio effect, and the traditional IS-LM analysis is adequate to describe bond-financed government deficits.

If portfolio crowding out does occur, in general it can . . . offset more than all of the standard income effect of fiscal policy.¹²

The wealth coefficients refer to the partial derivatives of the demand for money and separately, the demand for capital, both with respect to wealth. One benefit from Friedman's synthesis is that it ties the portfolio-balance argument neatly to the more conventional IS-LM approach.

Since the primacy of monetary policy is a hallmark of monetarism,

the next task is to assess the links between the portfolio relationships assumed by monetarists, on the one hand, and their microeconomic neo-classical foundations--if any--on the other. But first, a look at the portfolio-balance method in general.

The Price of Capital

Crude Keynesian practice is to make the rate of investment a function of the interest rate. However, an authentic microeconomic approach would denounce this as an ad hoc procedure.¹³ What really varies with the rate of interest is not the rate of investment, but rather the capital stock desired by the firm, and therefore in the aggregate by all firms. Correct economic calculation starts from a comparison of the cost to the firm (or the price) of an additional unit of capital with the change in total revenue which accrues to the firm if it acquires the additional unit of capital. The former is a stock variable, but the latter is a flow, which stretches over time--over the life of the capital good. The increase in revenue has to be converted into a stock, to make possible a comparison with the increase in cost. Here, we do use the rate of interest but not directly in the determination of the rate of investment, but merely to translate the extra flow of services into the stock-equivalent marginal benefit to the firm, and then use the latter to find the optimal capital stock. The transition from one such stock to the next is compatible with an infinity of possible rates of investment. Of the latter, the one which achieves flow equilibrium will be that for which the supply price of capital equals the market price. Complete, stock-flow equilibrium is achieved with stock equilibrium on the demand side, flow equilibrium on the supply side.

The latter would be taken care of, in a general equilibrium model, by the theory of the firm, and in this case, the firms involved would make up the capital-producing industry. Macroeconomic models such as Brunner and Meltzer's which, as mentioned above, separate the "aggregative" from the "allocative," stop short of any microeconomic modelling. But the inclusion of both the interest rate and the price of capital as the stations through which the transmission channels operate is a more correct procedure, from a neoclassical microeconomic standpoint, than if only the interest rate were considered.

The use of both the price of capital and the interest rate as indicators of policy is not unique to monetarists. Such Keynesians as Tobin and B. M. Friedman use this procedure too. This is another example of the tendency among many Keynesians to minimize, or to compensate for their deviations from the neoclassical. At the same time, its use by monetarists reflects their tendency to follow the neoclassical tradition.

Our next task is to address the broader question: is there a neoclassical precedent for the monetarist assertion of the short-term primacy of monetary policy?

Neoclassical Theory and the Short-Term Primacy of Monetary Policy

There is an ideological connection between the neoclassical microeconomic model and the potency attributed by monetarists to monetary policy. Neoclassical theory with its favorable treatment of market processes lends itself quite easily to the arguments of the political right. Activism in fiscal policy is not usually the province of right-wing conservatives, who tend to view its distributional non-neutrality

less favorably than they would view the consequences of monetary policy. To the extent that the right wing react negatively to all Keynesian claims for fiscal policy, there is a superficial, political link between the neoclassical school and monetarist view of monetary policy.

But what about a theoretical connection? On one count, there is an important theoretical link. In the neoclassical microeconomic model, monetary changes are exogenous. So also in the monetarist model. And one of the portfolio-substitutability assumptions of monetarism emerges automatically from the assumption of exogeneity. A close link between the money and credit markets would bring about a Radcliffean weakening of central bank control over the supply of monetary or money-like aggregates. That kind of proximity between the two markets is excluded by monetarism.

Monetarism presumes instead a portfolio closeness between the money and real capital markets. But the assumption of a perfect capital market places the equity market squarely in the real sector.¹⁴ Although equity holdings, like securities, are financial assets, sales and purchases of equity are regarded in portfolio theory as tantamount to purchases and sales of the physical capital which the equity lays a claim to. Put differently, portfolio theory does not recognize any distinction between the ownership and management of firms and the real capital inside them. Therefore the assumption of portfolio proximity between money and equity is simply a restatement of the presumption of the strong influence of money on the real economy. Unlike the portfolio separation of the money and capital markets, this assumption is not rooted in neoclassicism. As will be emphasized below, neoclassical microeconomic theory, while stressing the long-term quantity

theory, said nothing about the short-term effects of monetary policy. The short-term predictions of monetarism can have little to do with neoclassical microeconomic theory.

The manner in which the assertion of the exogeneity of money becomes translated into specific portfolio relationship in the monetarist model suggests the compatibility of monetarism with the neoclassical model. Can one go a step further and state that the relative potency or reliability of monetary policy is a necessary product of any macroeconomic theory rooted in the neoclassical tradition, such as monetarism? In other words, is this characteristic of monetarism, like the first three characteristics, an essential result of the hard core and the positive heuristic of the parent program? Or is it merely compatible with them, and can it therefore be relegated to the protective belt? The latter characterization is, I believe, the correct one.

It should be emphasized that the neoclassical microeconomic model is entirely a study of long-run comparative statics, while monetarism deals primarily in the short-run periods in between. It will be recalled that the neo-Walrasian model in Chapter 2--or any other neoclassical general-equilibrium monetary model, for that matter--has nothing to say about what happens between one equilibrium and another. And it is this period which sees the macroeconomic transmission process unfold. The neoclassical microeconomic tradition has no bearing on any macroeconomic conclusions concerning the potency or reliability of monetary or fiscal policy in the inter period.

To summarize, the monetarist use of the portfolio balance approach follows the neoclassical tradition, particularly in its use of the price of capital as an additional indicator of policy and in its

separation of the money from the credit markets. But the conclusions concerning the relative potency of monetary policy depend on more than these considerations. They also depend on short-term wealth effects of monetary and fiscal policy, the short-term income effects and additional short-term aspects of portfolio substitutability. About these, and about the short-term potency of monetary policy, neoclassical microeconomic theory has nothing to say.

The section entitled "New Research Programs" below, will suggest that a conservative macroeconomic program includes monetarism as a constituent theory. The neoclassical bias and the macroeconomic orientations are crucial ingredients of the program, and they are therefore reflected in the program's hard core and positive heuristic. But the monetarist assertion of the short-term primacy of monetary policy is not, and is therefore relegated to the protective belt.

Crowding Out and Ultrarationalism

A more detailed look at the IS-LM model sheds further light on the relative strength and reliability of fiscal and monetary policy, and will also serve as a transition to our next topic--the rational expectations hypothesis. In the IS-LM setting, one can distinguish between three separate effects at work. Take a bond-financed increase in government expenditure. The first is the direct fiscal effect on aggregate demand, represented by an expansionary rightward shift in the IS. There there is the crowding out of private investment as the interest rate rises along the LM (following the shift in the IS). Thirdly, the increase in the public holdings of government securities prompts wealth-induced shifts in both the IS and the LM. The shift in the former is expansionary, that in the

latter is contractionary. With all these conflicting effects, the net result is hard to determine on a priori grounds, especially if one's objective is a comparison with the effectiveness of fiscal policy.

Yet Blinder and Solow insist on an a priori solution, and set up an IS-LM model, complete with wealth effects and a C. Christ-style government-budget constraint. On the basis of their model, they concede that judging by the first two effects, bond-financed fiscal policy is weaker than monetary policy. Income will rise by less, as will tax receipts by government (at existing tax rates). And for that very reason, and also because bond-financing commits the government to future interest payments, "the budgetary gap is harder to close. . . . It therefore takes a greater rise in income to induce tax receipts sufficient to close the budgetary gap." They conclude that "not only is deficit spending financed by bonds expansionary in the long-run, it is even more expansionary than the same spending financed by the creation of new money."¹⁵

Unfortunately, their definition of the long-run--merely requiring that the government's budget be balanced--is likely to leave monetarists unconvinced of Blinder and Solow's conclusions. Had the "long-run" incorporated notions of taxpayer rationality and of the economy's automatic tendency toward full-employment or a "natural" rate of unemployment, the conclusions would have been very different, and more akin to those reached by the Ultrarationalists.¹⁶ The latter hold that the private sector is automatically crowded out by an increase in government expenditure, regardless of how it is financed. The private sector correctly anticipates future tax liabilities which inevitably will result from increased government borrowing. As a result of this

extraordinary degree of rationality on the part of taxpayers, fiscal policy leaves aggregate income unchanged. Ultrarationalism is to active fiscal policy what the rational expectations hypothesis is to active monetary policy.

The Rational Expectations Hypothesis

The concept of money illusion helps us to understand, from a neo-classical point of view, a major Keynesian departure from neoclassicism. It refers to the tendency of economic agents to calculate in nominal rather than in real terms--to measure their income, for example, in monetary terms rather than in terms of the claims it represents over physical products. The term "money illusion" implies that the use of money clouds the perception, as it were, and hampers the ability of the economic agent to measure variables accurately. Expansionary monetary policy, for example, would raise prices, but the money illusion would keep employees from asking for a higher wage. Whether economic agents, if they had the requisite information, would reason in real terms is beside the point. To a neoclassicist, the crucial point is that their behavior is irrational: neoclassical theory uses the substantive form of rationality and that assumes the transmission of adequate information.

The postulate of zero homogeneity of real variables in prices and income, and therefore the equation of exchange and the principle of the neutrality of money, went out the window with Keynesian economics. It could not longer be argued that an increase in the stock of money leads to a proportionately equivalent increase in all prices. Rather, expansionary policy could change relative prices (thanks partly to money

illusion) and therefore employment and output. However we observe in the literature various attempts at salvaging the homogeneity postulate and reconciling the Keynesian to the neoclassical. For example, some would maintain that the homogeneity postulate applied to all variables measured at their actual levels, but that some aspects of behavior were dependent upon expected levels. In other words, a basic premise of neoclassicism was compromised in order to gain acceptability by Keynesians. Some of the more eloquent attempts at the Keynesian-neoclassical reconciliation can be found in the literature of Phelps and company.¹⁷ An example is Clower's explanation of the Phillips' relationship between inflation and unemployment, an explanation also used to reconcile the monetarist vertical version of the Phillips curve to the conventional, downward-sloping Keynesian interpretation.¹⁸ The explanation revolves around a disparity between employers and employees with respect to the speed at which they adjust their expectations about the rate of price inflation. Employees' expectations change more slowly, and as a result their wage-demand lags behind the real wage as correctly (or, at least more correctly) perceived by the better-informed employers. The consequent increase in employment appears to be at best a short-run remedy, but then the Keynesian configuration never asked for more. The Keynesian objective is to try to stabilize cyclical fluctuations, and after all, cycles are short-run phenomena.

The point being made here is that much of the theoretical adjustment to neoclassical theory was in the presumptions about psychology of behavior. The major development in this context was the adaptive expectations hypothesis. According to the latter, expectations adjust gradually to actual values, with the change during period t in the

expected level of a variable being a constant function of the difference, in period t (or, alternatively, $t-1$) between the actual and the expected value of the variable.

In contrast, there is the rational expectations hypothesis (REH), which totally obliterates the potential for activist policy. The REH predicates that expectations are formed in exactly the same way as the theory makes its predictions. The appellation "rational" is widely accepted among economists, and the task of this section is to interpret the meaning of the term and to relate it to our earlier concept of substantive rationality (Chapter 1). It will be argued below that the REH ignores the complicated procedure in which mass psychology forms its expectations and replaces it with the presumption that theoretical predictions and mass expectations are one and the same. This is not to imply that merely because it uses the procedural type of rationality (also in Chapter 1), the adaptive expectations hypothesis is necessarily correct. However, by its very ad hoc nature, it at least recognizes the complicated nature of the matter and makes a weak attempt at an approximation.

To elaborate on the REH, consider this simple model put forward by Muth,¹⁹ who was inspired by the observation that "the average of expectations in an industry is more accurate than naive models and as accurate as elaborate equation systems." Let C stand for consumption and Y for production; let p represent prices, p^e expected prices, all measured as deviations from their respective equilibrium levels. Then suppose consumption is a function of current prices, while production is determined by expected prices:

$$\begin{aligned}
 C_t &= -ap_t \\
 Y_t &= bp_t^e + u_t \\
 Y_t &= C_t
 \end{aligned}$$

The term u stands for the error term. From the above, it follows that

$$p_t = -\frac{b}{a}p_t^e - \frac{1}{a}u_t$$

With $Eu_t = 0$ and no serial correlation, the price level predicted by the theory is

$$Ep_t = -\frac{b}{a}p_t^e$$

where, again, p^e represents the price level expected by economic agents.

Muth adds that, "Information is scarce and the economic system generally does not waste it"; it follows that, "if the prediction of the theory is substantially more accurate than the expectations of the firm, . . . there would be opportunities for the 'insider' to profit from the knowledge," until the "aggregate expectation of the firms is the same as the prediction of the theory."²⁰ In other words, $Ep_t = p_t^e$, which (assuming $b/a \neq -1$) will be true only if $p_t^e = 0$. This means that the price expected by the public will be the equilibrium price. This procedure drastically simplifies the problem at hand. Were it not for the assumption of rationality, $Ep_t = p_t^e$, and the consequent $p_t^e = Ep_t = 0$, the theorist would face the task of figuring out how expectations are formed: Ep (the price predicted by the theory) would depend on the manner in which p^e (the price expected by the public) is generated. With a serially correlated error term, Muth shows that the price expected by the public becomes a geometrically weighted moving average of previous prices:

$$p_t^e = \frac{b}{a} \sum_{j=1}^{\infty} \left(\frac{a}{a+b} \right)^j p_{t-j}$$

and "the 'coefficient of adjustment' in the expectations formula [depends] on the demand and supply coefficients."²¹ This scenario is not possible under the REH, since persistent error as manifested in a serially correlation error term is certainly at odds with rational expectations.²²

The implications of this type of model for macroeconomic theory are very serious. For an active policy to be effective, expected prices must lag behind policy-manipulated, actual prices. This means that the authorities must aim at and achieve a growth rate in prices which outstrips expectations. If the expectations of economic agents are rational (in the REH sense), actual and expected prices would be identical, and policy would therefore be fruitless.

Originally proposed by Muth in 1961, the rational expectations appeared under a new name in 1970: Burmeister and Dobell called them myopic expectations, but the new name never caught on.²³ And in a sense, rational expectations are myopic. To see this, take adaptive expectations once again. Here the expected value of a variable is formed on the basis of present and past values of the variable, according to some system of weighting. As the weight attached to the present period increases, expectations become closer and closer to the rational in that at the limit, when the current actual value has a weight of one (and therefore the formation of expectations is myopic), expected and actual values are identical. Expectations adjust instantaneously and smoothly to reality so that the actual, expected and predicted levels of a variable become one and the same.

Yet the simplicity of this logic hides a potential flaw. A layman would point out that various theorists, each holding to a different theory about how the economy works, could all use the rational expectations hypothesis in their models. Expectations would then be formed in a different manner in each one of them, and yet they would all be "rational." This argument would be trivial were it not for an underlying problem which it reflects. The REH distinguishes between the predicted level of a variable and its expected level, whereas there are really three: the actual level, the level predicted by the theory and the level expected by economic agents other than the theorists. The REH assumes that the expected level equals the predicted level of the true theory. Misspecification is assumed to be randomly distributed around the true theory. Two conditions need to be met for such a perfect market in information to emerge: (1) From among the theories tested by economists, the "best" one will emerge and become accepted throughout the economy. (2) Since the actual and the predicted values must be the same, the "best" theory must also be the perfect theory. This requires that the available information must be adequate for the "best" theory to be the "correct" one too.

Benjamin Friedman²⁴ writes that there are essentially two "information requirements." The first is the information exploitation assumption, which asserts that "people will exploit information until the point at which its marginal product equals its (perhaps zero) marginal cost." The second is the information availability assumption, which requires that "people not observe or know in advance the value of certain economic variables, but also draw, on the basis of these known values, inferences which are identical to the inferences of the process

actually generating the outcomes in question." This second assumption boils down to the following two statements, (1) economic agents have access to the relevant data, and (2) economic agents either have the correct economic model or they generate predictions as if they had the correct model.

It is now time to return to the distinction made earlier (back in Chapter 1) between procedural and substantive rationality. The complex problems faced by econometricians in their day-to-day work are an example of the procedurally rational technique, where the objective is to devise and implement a way in which (or a procedure by which) to achieve some objective, in this instance that of generating accurate predictions. Likewise, the person who is about to make an economic decision might have to make up his mind as to which of the currently available forecasts to use. The REH, on the other hand, uses the substantive type of rationality. In our earlier terminology, the problem is set up in a single-exit fashion, where economic activity in the formation-marketing system ensures that the economic agent has a choice between the perfect and correct forecast and all the incorrect ones, and the decision is, very predictably, to choose the former.

In the latter situation, any form of policy is doomed to failure. For example, as mentioned above, monetary policy could be effective only as long as it is able to open a gap between actual and predicted values. It turns out, however, that an implication of the rational expectations hypothesis is that even in the short-run, any deviation between actual and expected values will consist purely of the so-called white noise. In other words, any such deviation is totally independent of policy. As a result, active policy is impotent, even in the short-run.²⁵

This brings us back to the axiomatic, neoclassical world above, where there was no short run and there was no room for policy in what was an entirely microeconomic world. The macroeconomic short-term policy niche which has been of concern to economists particularly since Keynes does not exist.

Some economists have attempted to find room within this sort of theory for an effective short-term policy. In one such attempt,²⁶ it has been noted that in econometrics, the estimate of a parameter will approach its true value but only as the sample size increases. Econometricians often use the rolling-sample technique, where older data are discarded as newer data becomes available. The justification for this procedure is that the specification of any model is only an approximation of the true economic structure and that such an approximation becomes less and less valid as the time frame expands with the sample size. The consequent need to limit the time-span implies that the econometrician might never achieve a sample size large enough for the estimate to ever "reach" the true value. There is a need to limit the sample size, and yet there is also a need to have a large enough sample size. It follows that the perfect model, and therefore truly rational-agent expectations are unattainable; this is then used to justify a hybrid form of adaptive expectations which would allow for a short-run effectiveness of policy.²⁷

What makes all this particularly relevant for our purposes is the apparent contradiction between substantive rationality and macroeconomic theory. The relevance of macroeconomics is achieved only as the theory moves from the substantively to the procedurally rational, away from perfect rationality and the rational expectations hypothesis, a

transition which appears to run parallel to the move from the micro-economic to the macroeconomic.

Monetarism and the REH

Monetarism has traditionally presumed that expectations take a while to catch up with the economic consequences of a change in policy. Hence the presumed initial impact of monetary expansion on real variables, such as output, and only later on the price level. The reason for this is that in the aftermath of an expansionary policy spurt, agents may expect an unchanged inflation rate and therefore the public reacts by increasing real output. However, since the unemployment rate cannot be reduced below its natural rate, this real expansion is short-lived. But this interregnum where the policy-makers get away with a reduction in unemployment, while recognized by monetarists, is not compatible with the REH, where the expected price level does not wait before catching up with the actual price level.

But how significant is this difference between monetarism and the REH? Monetarists generally recognize the short-term potency of policy, but still do not recommend the use of activist, interventionist, policy for short-term or any purpose. Because of the indeterminacy of time lags and the fuzziness of the transmission mechanism, activism will not attenuate but instead will worsen the ups and downs of the business cycle. In the short-term, unemployment may be reduced, but the overall destabilization might end up worsening the underlying, natural rate of unemployment. To that extent, policy intervention is not futile but indeed harmful. On this count, then, I would conclude that there is no operational difference between monetarism and the REH.

But, on another count, a significant difference appears. Monetarism, despite its microeconomic foundations, is a macroeconomic science. In a sense, as Mayer argues, it is more aggregative than Keynesianism. On the other hand, the REH is minimally macroeconomic. If adherents of the REH had their way, macroeconomics as we know it would disappear. Given the confrontation with the Keynesians, macroeconomic equipment is *de rigueur*. But it should come as no surprise that literature emanating from the Federal Reserve Bank of Minneapolis makes a stir for a return to the general equilibrium model. Wallace lashes out at both monetarist and Keynesian econometric models, accusing both of being partial-equilibrium models, little more than a hodgepodge of correlations, with the ad hoc theorizing lacking the proper theoretic-behavioral foundations. This allegedly shows up in economic agents persistently committing the same mistakes, as in the formation of expectations in the adaptive-expectations formulation in such models.

. . . [It] could be the case that the structure during the sample period (including government policy) was such that the best forecast of the future price level is the current price level--often called static expectations. But such a scheme would not be best if for whatever reason, possibly a different government policy, the price level turns out to increase as, say, 7% per year. A model that implicitly assumes that people forecast as if the price level takes a random walk around a zero trend when, in fact, it has a nonzero trend is a disequilibrium model.²⁸

What the REH does, of course, is to apply the equilibrium concept to the generation of expectations. With regard to the problems in macroeconomic models, Wallace advises against tinkering with the available models, and recommends a wholesale switch to "microeconomic general equilibrium theorizing and modeling."

One can easily foresee the upcoming swing in academic economics

back toward substantial microeconomic detail. What will perhaps distinguish these future models is the recognition that analysis of policy calls for an emphasis on the stochastic dimension of the relevant problems.

In sum, then, the natural tendency for a discussion of monetarism to extend into the REH follows for a variety of reasons. The REH may be interpreted as a restatement of various aspects of monetarism, but in stronger terms. There is in both the same denunciation of policy activism, the same allegiance to the equation of exchange. Politically and ideologically they are stablemates. But the role of microeconomics is far greater in the REH. Monetarism was a shot in the arm for macroeconomics, particularly after the Keynesians ran into difficulties. The macroeconomic tradition may be in trouble if REH spreads around.

New Research Programs

It is now time to fit monetarism and the REH into their appropriate research programs. The macroeconomic character of monetarism suggests that the neoclassical microeconomic program of Chapter 1 cannot contain monetarism as a constituent theory. But that same program contains no propositions which are inconsistent with the REH, which can therefore be regarded as one of the theories generated by the positive heuristic of that program.

Monetarism, like Keynesianism, fails the first postulate of the Latsis' neoclassical microeconomic program on page 15. Economic agents cannot be presumed to be omniscient in a monetarist model, unless these agents are understood to be macroeconomic agents, or policy-makers. (This is, in fact, where monetarism differs from the REH.) It has to

be stressed that the microeconomic agent of Latsis' neoclassical research program is a different character from the agent of monetarism, who is primarily the macroeconomic policy-maker.

The research program whose constituent theories include monetarism is also not the Keynesian one of Chapter 3 (page 66). In particular, the policy suggestions of one program differ significantly from those of the other: they are truly competing programs. The monetarist program acknowledges the well-functioning nature of market processes: the Keynesian program does not and the absence of such an acknowledgement is evident in the hard core and positive heuristic of the Keynesian program and needs to be rectified in the construction of a rival program. So a conservative macroeconomic research program is suggested below, from which monetarism is spun off as one member theory. This new program retains from Latsis' neoclassical microeconomic program the emphasis on the stability and well-functioning nature of the market mechanism. But it does compete with the same program in that it uses a different, macroeconomic, theoretical equipment. It borrows this apparatus from the Keynesian program, but it differs from the latter in its regard for the market system, and it competes with it in its policy recommendations. It cannot be called a monetarism program, because it could possibly generate more than just monetarist theories. Instead, its label as a conservative macroeconomic program comes from its two major qualities which set it apart from competing programs.

Its hard-core reads:

(i) The market economy is inherently stable and self-regulating,
while its positive heuristic goes as follows:

(a) Construct macroeconomic models, equipped to analyze
macroeconomic policy issues, including those relating

to the interequilibrium period.

- (b) These models will be as compatible as possible with the microeconomic theory of general equilibrium.

As suggested above, monetarist predictions concerning the potency and reliability of monetary policy (usually made in the context of a comparison with fiscal policy) are not necessary products of the hard-core and the positive heuristic just predicted. Hence those particular predictions, while crucial to monetarism, belong only in the protective belt of the program. This particular point is discussed further in Chapter 5.

In contrast, the REH is not incompatible with any of the propositions in the hard-core of the neoclassical microeconomic program, and it meets every instruction of the positive heuristic of that program. The decision-makers referred to in propositions (i), (ii) and (iii) of the hard core can now be interpreted as being either microeconomic or macroeconomic. All that is required is that the term "decision-makers" be less rigidly interpreted than in Chapter 1. The substantive rationality with which microeconomic agents are endowed in the neoclassical program, and the ensuing situational determinism, come out intact in REH: propositions (b) and (c) of the positive heuristic of the neoclassical program, like all the other components of that program, are fully honored by the REH. It is, of course, true that the REH is generally discussed in the context of the debate about macroeconomic policy but this does not make it a macroeconomic theory. For one thing, the logical implication of the REH is that there is no interequilibrium period, no short run: this automatically disqualifies the REH as a macroeconomic theory, in the conventional interpretation of the word. The REH has, of course, a

major macroeconomic implication in the sense that it leaves no scope whatsoever for activist macroeconomic policy. But its use in macroeconomic models is not different from any other use of the implications of the neoclassical general-equilibrium theory in a macroeconomic discussion. The REH is essentially a rewriting of the Walrasian general-equilibrium model, with added emphasis on issues relating to the transmission of information.

Chapter Notes

- ¹On this first characteristic, see J. Stein, "Introduction, the monetarist criticism of the new economics" in J. L. Stein, Ed., Monetarism, Amsterdam: North Holland, 1976, p. 2; T. Mayer, The structure of monetarism, New York: Norton, 1978, pp. 14-15; H. R. Vane and J. L. Thompson, Monetarism: theory, evidence and policy, New York: Wiley, 1979, pp. 14-15.
 On the characteristics of monetarism in general, see also R. T. Selden, "Monetarism," in S. Weintraub, Ed., Modern economic thought, 1977.
- ²M. Monti, "Discussion of chapter 2" in M. Monti, Ed., The "new inflation" and monetary policy, London: Macmillan, 1976.
- ³H. R. Vane and J. L. Thompson, op. cit., 1979, pp. 15-16; T. Mayer, op. cit., 1978, pp. 15-25. Also; F. Modigliani and A. Ando, "Impacts of fiscal actions on aggregate income and the monetarist controversy: theory and evidence" in J. L. Stein, Ed., op. cit., 1976, particularly pp. 30-42.
- ⁴H. G. Johnson, "A survey of theories of inflation" in Essays in monetary economics, Cambridge: Harvard University Press, 1967, p. 123.
- ⁵M. Blaug, Economic theory in retrospect, 3rd edition, Cambridge: Cambridge University Press, 1978, p. 646.
- ⁶T. Mayer, op. cit., 1978, p. 24.
- ⁷J. L. Stein, op. cit. in J. L. Stein, Ed., 1976, pp. 6-10. Friedman's studies in the long-run Quantity Theory have not precluded his analysis of short-term monetary-transmission issues. See, for example, M. Friedman, Optimum quantity of money, and other essays, Chicago: Aldine Publishing Co., 1969, pp. 177-181 and M. Friedman, A theoretical framework for monetary analysis, N.B.E.R. Occasional Paper, New York: Columbia University Press, 1971, pp. 51-53.
- ⁸M. Friedman, Essays in positive economics, Chicago: University of Chicago Press, 1953.
- ⁹K. Brunner, "Inflation, money and the role of fiscal arrangements: an analytic framework for the inflation problem" in M. Monti, op. cit., 1976. Also relevant are K. Brunner and A. H. Meltzer, "Mr. Hicks and the 'monetarists'," Economica, February 1973, pp. 44-59; Brunner and Meltzer, "Money, debt and economic activity," Journal of Political Economy, 1974, 80, pp. 951-977.
- ¹⁰K. Brunner, ibid., 1976, p. 39.

- ¹¹J. Tobin, "Friedman's theoretical framework" in R. J. Gordon, Ed., Milton Friedman's monetary framework: a debate with his critics, Chicago: University of Chicago Press, 1974 (Quoted in J. L. Stein, op. cit., in J. L. Stein, Ed., 1976, p. 8). On the portfolio balance approach, see also V. Chick, The theory of monetary policy, revised edition, Oxford: Basil Blackwell, 1977, Chapter 6.
- ¹²B. M. Friedman, "Crowding out or crowding in? Economic Consequences of financing government deficits," Brookings Papers on Economic Activity, 3:1978, p. 629.
- Using a broader interpretation of the scope of the issue, T. Mayer, op. cit., 1978, p. 13, writes that there are:
 four links between the hypothesis of the primacy of changes in the quantity of money and the monetarist--as opposed to the Keynesian--version of the transmission process. One is the stability of the demand for money, the second is the relative measurability of money and interest rates, the third is the range of assets considered, and the fourth concerns the relative price effects and stock effects discussed by Brunner and Meltzer.
- The first two--a stable money demand and the adequate measurability of money--follow automatically from a belief in a stable real economy and an exogenous money supply (i.e., a money supply which is exogenous to the model, but endogenous to the central bank). The fourth link--the Brunner and Meltzer contribution--is precisely what Friedman is discussing here. The third link--the range of assets under consideration--is usually discussed in the context of the IS-LM and involves the question of the proper definition of consumption, saving and investment, and the question of whether monetary influences operate only through investment or through investment as well as consumption, and so on. Friedman relates the IS-LM discussion to the broader portfolio-balance approach.
- ¹³This section reproduces some of the ideas contained in Brunner, op. cit., 1976, pp. 31-33, and in J. G. Witte, Jr., "The micro-foundations of the social investment function," Journal of Political Economy, 1963, 71, No. 5, pp. 441-456.
- ¹⁴v. Chick, op. cit., 1977, pp. 100-101.
- ¹⁵A. S. Blinder and R. M. Solow, "Does fiscal policy matter?," Journal of Public Economics, 1973, 2, pp. 319-337.
- ¹⁶W. H. Buiter, "'Crowding out' and the effectiveness of fiscal policy," Journal of Public Economics, 1977, 7, pp. 309-328.
- ¹⁷See, for example, E. S. Phelps, Microeconomic foundations of employment and inflation theory, New York: Norton, 1970.
- ¹⁸R. W. Clower, "Some theory of an ignorant monopolist," Economic Journal, December 1959, 69, pp. 705-716.

- 19J. Muth, "Rational expectations and the theory of price movements," Econometrica, June 1961, 29, No. 3, pp. 315-335.
- 20J. Muth, ibid., p. 318.
- 21J. Muth, ibid., p. 320.
- 22T. S. Sargent and N. Wallace, "Rational expectations and the theory of economic policy," Journal of Monetary Economics, 1976, 2, pp. 169-183.
- 23E. Burmeister and A. R. Dobell, Mathematical theories of economic growth, New York: Macmillan, 1970, p. 163. See also J. L. Evans and G. K. Yarrow, "Adaptive and rational expectations in macro-economic models," Manchester School of Economic and Social Studies, 1979, 1, pp. 24-32.
- 24B. M. Friedman, "Optimal expectations and the extreme information assumptions of 'rational expectation' macromodels," Journal of Monetary Economics, 1979, 5, pp. 23-41.
- 25T. S. Sargent and N. Wallace, "'Rational' expectations, the optimal monetary instrument and the optimal money supply rule," Journal of Political Economy, 1975, 83, No. 2, pp. 241-254.
- 26B. M. Friedman, op. cit., 1979.
- 27See P. Miller, C. Nelson and T. Supel, "The rational expectations challenge to policy activism," in Federal Reserve Bank of Minneapolis, A prescription for monetary policy: proceedings from a seminar series, 1976; L. Weiss, "The role for active monetary policy in a rational expectations model," Journal of Political Economy, 1980, 88, No. 2, pp. 221-233; J. B. Taylor, "Monetary policy during a transition to rational expectations," Journal of Political Economy, 1975, 83, No. 5, pp. 1009-1021.
- 28N. Wallace, "Microeconomic theories of macroeconomic phenomena and their implications for monetary policy" in Federal Reserve Bank of Minneapolis, op. cit., 1976.

CHAPTER 5

CONCLUSION

In economics, theory is tested not merely by its predictive ability (broadly speaking, the type of testing shared with the natural sciences), but also in the course of time by the degree of political success earned by the policies inspired by the theory. From that follows the procedure of distinguishing between theories not only on the basis of the nature of their theoretical method (for example, whether they are macroeconomic or microeconomic), but also by the political flavor of their policy recommendations.

No theory can be all-inclusive: comprehensiveness is cut down as the theorist selects, on a priori grounds, the more important variables, and between these the more vital relationships. Keynesian theory operates primarily at the macroeconomic level, while the more ideologically conservative theories tend towards the microeconomic end of the spectrum, and automatically make stronger assumptions about the rationality of economic agents. The degree of detachment from the macroeconomic as well as the severity of the psychological assumptions increase as the theory veers away from the study and formulation of short-term activist policy.

Monetarism has very few good words for macroeconomic activism. Still, its tools of analysis are principally macroeconomic and one of its major concerns is the role of monetary processes in the periods

between equilibria--a period where microtheory leaves a void--and its assumptions about microeconomic rationality are much less rigid than those of neoclassical microeconomic theory. For these reasons, Chapter 4 made monetarism a member of a conservative macroeconomic program.

The rational expectations hypothesis, on the other hand, leaves no room whatsoever for policy activism, and in line with the pure theory of comparative statics has no short run. Like the ultrarationalist school in fiscal theory, the REH has dramatic implications for macroeconomics: to wit, macroeconomic policy is totally futile. It does not breach any of the propositions of Latsis' neoclassical program, and it was identified in Chapter 4 as a member of that program. Like microeconomic general-equilibrium theory, it manifests situational determinism at the level of the microeconomic agent: the substantive form of rationality is used in the generation of expectations according to the rules of the REH. The hypothesis itself generates some macroeconomic implications of the general equilibrium model, and what makes these macroeconomic conclusions stand out is the situational determinism at the microeconomic level, a determinism brought about by stringent assumptions about the psychology of microeconomic agents.

If one defines macroeconomics simply as the study of aggregate variables, the REH can be considered macroeconomic. If however one uses a neoclassically-oriented definition of macroeconomics--as the study of the macroeconomic-policy determination of macroeconomic variables in the disequilibrium, short-run period--then the REH is not macroeconomic. In particular, it shows no concern with the short-run, which can be regarded from the neoclassical point of view as a province

of macroeconomics. Admittedly, it does concern itself with macro-policy, to the point of saying that macropolicy is entirely futile. But using Lakatosian terminology, one can suggest that the future of the REH, to the extent it stays at the forefront of a progressive neo-classical research program, lies in the elaboration of microeconomic, general equilibrium models.

This almost brings us back, full circle, to the neoclassical monetary model of Chapter 2, except that the latter not only stressed the futility of activist policy (as does the REH), but went a step further and asserted the irrelevance of all policy.

Situational determinism pervades mainstream economic theory. At the microeconomic level, the adoption of the substantive form of rationality leads to the single-exit solutions of microeconomics. From the neoclassical point of view, Keynesian macroeconomics makes much weaker assumptions about rationality. But even so, Keynesianism along with monetarism show a situational determinism at the macroeconomic, policy-maker's level. The problem is usually set up in such a way that the theory generates a clear-cut economic solution and the policy-maker implicitly faces a clear choice between the optimal policy and the wrong policy. The solution is strictly an economic one, and the theory abstracts away from all non-economic factors (such as political or social considerations) which might impinge on the policy-maker's freedom of choice and make the economically optimal solution no longer such an obvious choice.

Microeconomic situational determinism as used by Latsis defines the boundary between economics and psychology. The extension of the concept to macroeconomics throws the emphasis on the boundary separating

economics from the social sciences. On this boundary, a further comment is perhaps in order. Students are repeatedly reminded during their economics education of the vital distinction between positive or scientific economics, and untestable, value-laded normative economics. Yet during their careers most economists repeatedly make normative or prescriptive statements, of the type, "The federal government should . . ." or "The Federal Reserve must . . .," or, "the President is making a mistake in. . . ." Many of these recommendations are directed at political agents. Very often, these same agents interact through the political process (rather than through an economic or a market process, in the narrow sense), even though the outcomes of these processes are of an economic nature. But economists persist in drawing a line of demarcation between themselves and political scientists. It is of course true that a model which attempts to cover everything in social science is bound to be overburdened and worthless (in terms of its predictive ability), but perhaps the boundaries of macroeconomics are drawn somewhat too rigidly.

On the Lakatosian method, the reader will recall that this requires a precise definition of the program in the form of a set of hard core propositions and a set of instructions on how to construct theories within the program. This permits an accurate classification of theories by program with all the benefits that such precision entails. Membership in the monetarist program, for example, requires more than the prescription by the theory of a monetary rule. In linking monetarism to its precedents, Chapter 4 emphasized the compatibility of monetarism with neoclassical microeconomics--hence the right-wing ideological basis of monetarism. But Aschheim and Tavlas¹ deny that monetarism is

the "exclusive domain of conservatism," and support their argument by citing such writers in the Chicago tradition as Paul Douglas, whose writings show a "simultaneous allegiance to the quantity theory and to left-wing ideology."² However, by the criteria of the conservative macroeconomic research program formulated in Chapter 4, Paul Douglas was not right-wing enough to qualify as a monetarist, his recommendation of a monetary rule notwithstanding.

Stein writes, "conceivably (the monetarist) could be a Socialist but not a Marxist, because a monetarist believes that . . . the price system is an efficient allocation of resources."³ But since when do Socialists acknowledge the well-functioning of free markets? Obviously, there are brands of socialism which insist on monetary stringency and prudence, but that does not automatically entitle them to the label of monetarists. In Solow's⁴ lexicon, they perhaps fall under the category of "little monetarists," whereas in this dissertation, monetarism is described by its mainstream and logically-entailed features--what Solow belittles as the "syndrome" of "Great Monetarism."

The research-program methodology may also be useful as a predictive tool. It suggests that the propositions which are placed in the hard-core will be preserved as long as the program lives. In contrast, propositions in the protective belt are easily shed when they fail the test of empirical corroboration. Hence the value of the distinction between the essential characteristics of the conservative macroeconomic program and the propositions in its protective belt. Unlike the former, the latter are not essential to the survival of the program and may become modified, or even jettisoned, over time as the evidence dictates. Chapter 4 suggested that monetarism consists of a set of theories which

are members of a macroeconomic conservative research program, whose hard core and positive heuristic emphasize the macroeconomic nature of the analysis as well as its microeconomics theoretical underpinnings. I also suggested that the primacy afforded to monetary policy, while a major characteristic of monetarism, is not crucial to the program as a whole. Even though it is a primary characteristic of monetarism, the assertion of the potency and reliability of monetary policy (relative to fiscal policy) belongs in the protective belt of the program. The implication is that should the prediction of the primacy of monetary policy run into empirical difficulties, the conservative program may still survive, without that prediction, and perhaps without monetarism at its forefront. The conservative right would still have a macroeconomic program at their disposal, as well as the neoclassical microeconomic program.

Chapter Notes

- ¹J. Aschheim and G. S. Tavlas, "On monetarism and ideology," Banca Nazionale del Lavoro Quarterly Review, 129, June 1979, pp. 167-186.
- ²J. Aschheim and G. S. Tavlas, op. cit., 1979, p. 178.
- ³J. L. Stein, "Introduction: the monetarist criticism of the new economics" in J. L. Stein, Ed., Monetarism, Amsterdam: North Holland, 1976, p. 2.
- ⁴R. M. Solow, "Comments on Stein," in J. L. Stein, Ed., op. cit., 1976, p. 248.

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